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NOTES AND COMMENTS.

Mechanical Symmetry in Living Matter.

IN the various attempts to explain the shapes and structures of animals, one of the most curious recurrences is a comparison between crystalline structure and organic form. The forms of crystals depend upon the forms of the individual molecules, or groups of molecules, of which the crystalline bodies are composed, and upon the strains and stresses by which the molecules adhere together. The symmetry is the necessary outgrowth of the constitution and nature of the crystalline matter. Repeated attempts have been made to show that there may be a connection between the forms of animals and the constitution and nature of the supposed equivalent molecules, or groups of molecules, of which they may be composed. Some time ago we noticed an interesting volume by Dr. Haacke (Natural Science, vol. v., p. 66), in which he tried to show that the different shapes of animals depended upon different kinds of protoplasmic molecules. We have now received from Arturo Soria y Mata a pamphlet in which a similar theory is ingeniously elaborated. It is entitled *Origen Poliédrico de las Especies* (Madrid: Succesores de Rivadeneyra, Paseo de San Vicente, 20, 1894), and it attempts to show that organic forms may be reduced to aggregates of regular and irregular polyhedra.

We cannot follow the author at length into his curious enquiry. It seems to us to have been established that protoplasm, the basis of living things, is not a substance, but a mixture, and from the physical point of view we regard it as certain that no living body is composed of equivalent particles. It must be remembered, however, that the appearance of symmetrical patterns may come by the arrangement of dissimilar materials. A kaleidoscope elaborates regular and complicated patterns from dissimilar fragments of paper, glass, tinsel, and so forth. The pattern is produced by the complicated optical repetitions of the dissimilar units, each unit and the whole irregular group being repeated by mirrors. Now cell-multiplication produces a result closely similar. The multiplication of cells from a growing-

point forces out repetitions of the original cell in radiating directions, and, by the simple mechanical result of growth, points grow into stars or lines, masses grow into larger masses disposed about a centre. The formation of symmetrical patterns is retarded or distorted in many cases by the interference of adjoining patterns from adjoining centres of symmetry, and by the limitation of growth in fixed directions necessary to the production of definite organs. But in any structure built up by the repetition of parts an appearance of symmetry must be present. We are inclined to think that many of the theories of relation between body-form and protoplasmic molecule owe their origin to a neglect of the fact that pattern comes from arrangement, as well as from identity, of component parts.

REGENERATION AND INVOLUTION.

FROM their intrinsic interest, and from their importance as bearing upon problems of growth and heredity, the phenomena of the regeneration of lost parts and of the degraded forms of organs are of great interest. The additional attention that is now paid them may be seen from the bibliography for 1894, compiled by Dietrich Barfurth, and issued in the 1894 volume of Merkel and Bonnet's *Ergebnisse der Anatomie und Entwickelungsgeschichte*. Mr. Barfurth remarks on the presence of bitterness in the disputations, and on their occasional conduct with weapons unsuited to the parlour (nicht immer mit salonmässigen Waffen). In the work done in 1894 upon embryological regeneration conflicting results have been obtained. In most cases, observers found that isolated cells of the two-, four-, or eight-celled stages of the segmenting egg gave rise to complete embryos. These results, like the original experiments of Driesch, Hertwig, and Wilson, seem against the evolutionary views of Roux and Weismann. If, for instance, the first division of an egg divides it into a future right and left half, one would not expect a separated left half to reproduce the whole. Zoja, however, working on eggs of *Strongylocentrotus lividus*, found that isolated blastomeres gave rise to semi-morulæ, although Driesch, working on an allied family, found that they gave rise to complete morulæ. The most important of these papers is, perhaps, a critical summary of recent work by Roux, published in the *Archiv für Entwickelungsmechanik*. This valuable publication, we learn from the *Zoological Record*, may be consulted in the Linnean Society's library. Barfurth himself made an important series of investigations upon the regeneration of lost parts. He found, in the case of the young *Axolotl*, that complicated wounding and amputating of the limbs might result, not merely in regeneration, but in regeneration accompanied by the addition of supernumerary parts. The tendency to "super-regeneration" he found to be greater the nearer the proximal end of the limb the amputation was made. It is, so far as we know, the first case of polydactylism produced artificially.

The Transplantation of Living Tissues.

The original experiments of Hunter, in which he transplanted structures from one animal to another, probably led to the modern attempts at bone- and skin-grafting. A few years ago, surgeons were confident that grafts of bone from rabbits and calves might be transferred to human bodies, while it was a current belief that skin might easily be grafted, or blood transfused. Mr. H. G. Wells, whose scientific novels have been a feature of the last two years, has based the plot of his recent "Island of Dr. Moreau" on the artificial production of semi-human beings from animals. Dr. Moreau is a ferocious vivisector, with something of the hypnotist thrown in, and, by carving living animals (without anæsthetics) for many consecutive weeks, he has produced, and turned loose on his island, a set of amusing creatures, such as wolf-hyæna-men, ox-hog-men, goat-vixen-ladies, and a puma-dog-lady who escaped in an incomplete condition, to the subsequent destruction of her artificer. The story is gruesome and exciting to a high degree; but we have no doubt that our readers, who have missed great delights if they have not read the earlier scientific novels and stories of Mr. Wells, will form their own opinion of the qualities of the "Island of Dr. Moreau." From the scientific side, however, Mr. Wells seems to us to have allowed his imagination too free a run in his new story. In the *resumé* alluded to in our last Note, Mr. Barfurth sums up recent work on transplantation and transfusion conclusively against the success of operations conducted upon animals of different species. Transplantations from one species to another almost invariably have proved unsuccessful. Most often the transplanted pieces become centres of suppuration; in the most favourable cases, they serve as inert centres around which new growth takes place. Histological examination shows that they die. So extreme is the aversion of a body to extrinsic material, that transplantations from other individuals, even of the same species, rarely hold; they are treated as foreign bodies. The successes are almost entirely confined to plastic operations in which material from one part of a body is adapted to another part of the same body.

The Vestige of the Yolk-Sack.

Mr. Barfurth's section upon involution shows that rather less work has been published in 1894 than in many earlier years. One of the most interesting investigations was made upon the fate of the yolk-sack, by Strahl and Bersch. In the case of the blind-worm, it was found that the yolk-sack had completely disappeared six months after birth. In the wall-lizard, they found that the yolk-sack disappeared as a diverticulum of the gut, but was represented in the adult by a little mass of black pigment—another instance of the curious connection between degeneration tissues and the deposition of pigment. We do not recall any other observations on the vestige of

the yolk-sack among reptiles. Owen described it in a number of birds, declaring that the retention of a large rudiment was characteristic of water-birds. More recently, Mr. Chalmers Mitchell has shown that it is present in the adult forms of the greater number of the avian groups, and that it bears a constant relation to the disposition of the intestinal folds. There are few records of its occurrence among adult quadrupeds; but human anatomists are familiar with it as a not infrequent abnormality known as Meckel's diverticulum.

THE EVOLUTION OF ADAPTATIONS.

WE have received from Mr. G. D. Haviland a pamphlet entitled "Some Factors in the Evolution of Adaptations" (London: R. H. Porter, price sixpence). He endeavours to apply deductive reasoning to a set of propositions concerning asexual and sexual reproduction. So far as we can see, there is no flaw in his ingenious exercise; but as it happens, deductive reasoning is the last thing we want in biology. Everyone of us, though perhaps seldom with Mr. Haviland's skill, might sit down and deduce factors of evolution until the undertaker's man called for us, and science would be none the better for our labour. One of Mr. Haviland's "three axioms," for instance, is that the characters dealt with in his paper "exhibit heredity of deviation." If the writer were to spend a very small part of the leisure he is able to devote to this pastime of deduction upon the collection of new facts and the collation of old facts regarding the meaning and manner of the process he cheerfully calls an axiom, he might write something worth reading. When the axiom that two straight lines do not enclose a space is stated, a definite fact is stated. This axiom of Mr. Haviland, on the other hand, is a generalisation of the vaguest kind, based upon observations which range from absolute non-inheritance of particular deviations to complete inheritance. We want facts, not inferences, observations, not theories, for a long time to come.

THE ANATOMICAL SOCIETY.

WE hear that it is intended to hold a summer meeting of the Anatomical Society of Great Britain and Ireland at Oxford on Saturday, July 4. This Society, which was founded in 1887, meets, as a rule, four times a year, three of the meetings being held at the London medical schools in rotation, and the other at one of the provincial universities or schools. The aims of the Anatomical Society may fairly be said to come within the province of NATURAL SCIENCE, since it is instrumental in publishing a considerable number of papers on vertebrate morphology, as well as on pure anthropotomy. Among the papers which are likely to interest, or to have been of interest, to our own circle of readers, we may refer to the following: The Structure of the Vertebrate Liver, by T. W. Shore and Lewis

Jones; The Cerebral Hemispheres of *Ornithorhynchus paradoxus*, by Professor Sir William Turner; A Contribution to the Comparative Anatomy of the Ankle Joint, by Professor Cleland; The Pelvis of the Mammalia, with special reference to that of the young *Ornithorhynchus*, by Professor Howes; The Spine of the Young Gorilla, by Professor Symington.

In addition to publishing its own proceedings, the Society has an arrangement with the proprietors of the *Journal of Anatomy* by which its chief papers are published *in extenso* in that work, and are illustrated in a more complete way than would otherwise be possible. The Collective Investigation Reports, which should prove valuable to workers on variation, are also conducted by the Society; but perhaps the undertaking which is most likely to earn the gratitude of workers in zoology is the publication of a very complete index to the *Journal of Anatomy and Physiology*, an index which is already published for the first twenty volumes, and of which the remainder will be ready at the end of the present year.

Economic Work of the Marine Biological Association.

The recent number of the *Journal of the Marine Biological Association* (February, 1896) shows that an increasing amount of work of direct economic importance is being issued from the laboratory, in addition to the large output of abstract scientific work. It is, of course, impossible to distinguish exactly between researches that bear directly upon economic matters and researches conducted in the pure spirit of scientific inquiry; for the same methods and the same trained workers are required for both, and investigations apparently most remote from commercial questions may come to have an unexpected value. Mr. Bidder, for instance, has been pursuing investigations upon the embryology and anatomy of the Porifera for many years, and men of business might have looked coldly upon his nice discriminations between epiblast and hypoblast, or on his curious inquiries into the morphological nature of embryos. Yet when the Colonial Office applied to the Marine Biological Association for an opinion on the advisability of projects for improving the value of sponge-fisheries, Mr. Bidder's contributions to the report proved of the highest practical value. The whole report as presented to the Colonial Office is reprinted in the current number of the Association's journal. The Director and Mr. Bidder, on the whole, sum up against the proposals to improve sponge-fisheries by planting cuttings. They think that there is not sufficient evidence as to the rate of growth of the sponge of commerce, and that, unless the cuttings were exposed to conditions much more favourable by the construction of special stages, it is unlikely that a greater weight of sponge would be got in the same time from the cuttings of a sponge than from the original undisturbed sponge. They suggest that fishing in deeper water

should be tried, and that the importation of living sponges from the Mediterranean should be attempted.

In the same issue of the *Journal*, Mr. Cunningham continues the report of his valuable North Sea investigations. He deals with the size of mature plaice, turbot, and brill on different fishing grounds; with special observations upon the fish taken at Grimsby, Scarborough, Hull, and Lowestoft; with the causes of distribution of different-sized fish in different regions of the German Ocean; and with the influence of proposed restrictions on the sale of undersized fish. The report contains a large bulk of matter of great interest to naturalists and to the fishery trade. If we might venture to make a suggestion to Mr. Cunningham, it would be that he should attempt to sum up separately the conclusions which he sees to be of practical value. Practical people prefer results treated categorically, rather than philosophically.

Mr. Albrecht Bethe gives a preliminary note, with a figure, of an interestingly abnormal crab. To the left side of the fifth segment of the abdomen there was attached a walking-leg. Mr. Bethe promises a detailed account, with theoretical explanation of the abnormality, in a "German journal."

Preserving Fluids.

In reference to our Note last month on the preservation of molluscs, Mr. H. Hanna writes to us from Belfast that the use of cocaine, and of graduated alcohol after the production of complete anæsthesia, is not the only reliable method for molluscs. He has found a less complicated course "simply perfect, both as regards completeness of expansion and preservation of natural colour." An *Aplysia* was put from sea-water directly into a small basin filled with a 10 per cent. solution of chromic acid, and allowed to remain in that for from one to two hours. It was then removed and washed repeatedly, to get rid of the chromic acid, and passed through graduated alcohols. He has a specimen in spirit, treated as above last August, and now displaying the natural colours and fully expanded state of the living animal.

We learn that the use of formalin is becoming very general. It has been employed with success, both as an injecting and a preserving agency, in the Prosector's Laboratory at the Zoological Society's Gardens. It is used in the anatomy rooms and in the pathological departments of several hospitals in London, with varying success. It is reported as being unsatisfactory for the preservation of brains, as it renders them very brittle, and as it makes almost impossible the removal of the *pia mater* from the preserved specimens. Two practical objections are urged against its use. It has unpleasant effects upon the hands, as we ourselves are able to testify. It makes the skin hard and dry, and wrinkles the ends of the fingers, while it engrains dirt in an undesirable fashion. Moreover, we have heard several complaints of

its having set up throat irritation, less severe, but more prolonged, than that caused by chlorine fumes.

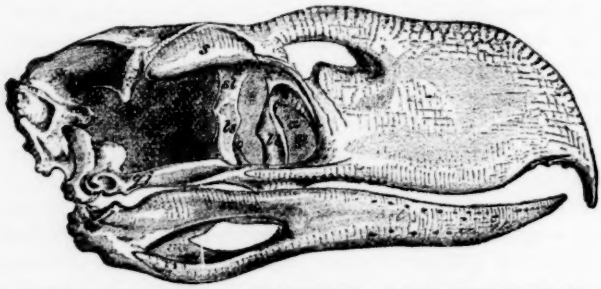
Papers on formalin are becoming numerous. Among them we notice one by O. A. Sayce, published in the *Victorian Naturalist* for December, 1895, pp. 101–104. The author has used formalin for the preservation of plants, and finds that the chlorophyll is not dissolved out as it is with alcohol. To facilitate the penetration of formalin into animals with a thick integument, he has adopted two plans with success, one is to kill by immersion in glacial acetic acid for a minute or two, or less, according to circumstances, and, without washing the acid out, to place the specimen at once in 5 per cent. formalin, washing the acid out on the following day with fresh 5 per cent. formalin. The other method is to heat the solution, which, however, weakens it by driving off the formic aldehyde.

Phororhacos at the British Museum.

Some months ago (Natural Science, vol. vii., p. 166; September, 1895) we drew attention to a paper by Dr. F. Ameghino describing a number of very remarkable fossil birds from the Santa Cruz beds of Patagonia. The whole of this collection has recently been purchased by the Trustees of the British Museum, and is now in part exhibited at the Natural History Museum, where it forms one of the most valuable and interesting additions that have been made to the collections during the last few years. Besides the remains of *Phororhacos* and several other "Stereornithes," it includes a considerable number of ordinary carinate birds of smaller size belonging to several families. Of the gigantic forms, *Phororhacos* is by far the best known, the greater part of the skeleton being represented in the collection. The mandible of *Phororhacos longissimus*, the largest species, is certainly one of the most remarkable specimens in the Museum, and it can easily be understood how Ameghino, with only a fragment to go upon, imagined that he was dealing with the mandible of a large edentate mammal. The nearly complete specimen measures about 56 cm. (22 inches) in length and 7 cm. ($2\frac{3}{4}$ inches) across the widest part of the symphysis; the whole is exceedingly massive. The skull found with it was about 2 feet in length, but unfortunately was so fragile that only some fragments were preserved. Of the smaller species, a complete skull and mandible, together with a great part of the rest of the skeleton, are shown. The skull (for a figure of which we are indebted to the editor of *The Ibis*) resembles that of a raptorial bird in its general aspect, and particularly in its powerful hooked beak, which, however, differs from that of Accipitrine birds in its great depth and compression. The quadrate of this specimen shows clearly the presence of two heads for articulation with the skull, and this character, together with many others in other parts of the skeleton, is evidence that these birds were not Ratitæ but Carinatæ

in which the power of flight had been lost. From the Ratitæ they also differ widely in the relative proportions of the head and body, for in the struthious birds the head is in all cases extremely small in comparison with the whole bulk of the body, while in *Phororhacos* it is proportionately very large. Probably the Shoe-billed Stork (*Balæniceps*) gives the best idea of these proportions in *Phororhacos*; the two are, however, in no way related, and the shape of their heads is quite different.

The bones of the fore-limb were all very short, but at the same time stout and strong, so that although these birds were almost certainly incapable of flight, they still possessed, for some purpose or other, a powerful wing. The pelvis has been to some extent crushed, so that the extreme narrowness of its post-acetabular region, as shown in Ameghino's figure, is somewhat exaggerated. All the bones of the hind-limb are known in this species, so that the height of the bird at the middle of the pelvis can be estimated to have been less than 1 metre. The skull is 34 cm. (about 13½ inches) long.



SKULL OF *Phororhacos inflatus*, Ameghino. One-fourth natural size.
(After Ameghino.)

As to the affinities of these birds there is still much doubt. It is clearly impossible to regard them as struthious, and their great specialisation renders it very difficult to ascertain their place among the Carinatæ. In a recent paper in *The Ibis*, Mr. C. W. Andrews has suggested that their nearest living ally is that curious South American bird the Cariama (*Dicholophus*), which, though much resembling the Secretary-bird, is now usually regarded as a very aberrant crane. To this, in some respects, as for example in the structure of the metatarsus, the resemblance of *Phororhacos* is very great. If this surmise be correct, it is a matter of some interest to find that some of the birds, like many of the mammals, now living in South America, have extinct representatives of gigantic size and a high degree of specialisation.

PROSSER ON PERMIAN PROBLEMS.

CONSIDERABLE doubt has prevailed as to the relations of the Permian beds in the Western States of America, if, indeed, the

Permian were represented there at all. Dr. Newberry, at all events, considered that the so-called Permian could not be separated from the Coal-measures. A careful and detailed paper by Professor Charles S. Prosser, of Union College, Schenectady (*Journal of Geology*, vol. iii., pp. 682–705, and 764–800; 1895), shows on incontrovertible fossil evidence that true Permian rocks do occur in Central Kansas. Waagen, in correlating the Upper Palæozoic strata of the Salt-Range (*Palæontologia Indica*, ser. xiii., vol. iv., pt. ii., p. 238), drew the dividing line between the Carboniferous and Permian systems of North America at the top of the "Upper Productive Coal-measures." Mr. Prosser considers that this line was placed too low, since the Wabaunsee formation of Kansas, which occurs above it, contains a fauna practically identical with that of the Upper Coal-measures. To that series he would refer both Wabaunsee and Cottonwood formations. The Neosho and Chase formations are transitional, but would be included by most European geologists in the Permian, while the Marion formation, which in Kansas is succeeded by Cretaceous beds, contains none but characteristically Permian fossils. The paper, owing to its minute detail and abundance of unfamiliar names, will not prove exhilarating reading for European geologists; but it will be of value to those of them whom necessity or inclination lead to make closer acquaintance with American stratigraphy.

Gneiss, Gold, Galena, and Garnets.

In an extract of 85 pages from the Sixteenth *Annual Report* of the Director of the U.S. Geological Survey, Part II., Mineral Resources of the United States, 1894, Dr. G. F. Becker gives a general reconnaissance of the gold-fields of the Southern Appalachians. The area contains three productive belts, the Georgian, the South Mountain, and the Carolinian. Of the two first, the rocks are chiefly Archæan gneisses and schists, sometimes intersected by granite dykes. The conditions in the Carolinian belt are more complex. Here the rocks are metamorphosed sediments (chiefly clay-slates) containing irregular patches of volcanic rocks with intruded basic dykes.

To the banded gneisses of the Georgian belt the author ascribes an eruptive origin, and considers that, as in the banded Tertiary gabbros of Scotland described by Geikie and Teall, the banding arises from the intrusion of heterogeneous magmas. The auriferous veins are in cracks running approximately parallel to the schistosity. The fact that galena is a common associate of the gold rather negatives the idea that the ore was brought by an alkaline solvent. The original source of the gold is still to seek, but in many cases there is evidence to show that the igneous dykes are connected with the deposition of ore. An interesting point is that the garnets in some of the garnetiferous schists are auriferous. As these garnets

crystallised in the schists after the schistose structure was developed, the author sounds a note of warning as to good idiomorphism being an infallible sign of the early genesis of crystals in igneous rocks.

ALPINE FLORA.

THE study of present conditions in the Arctic Regions is so necessary if one wishes to understand the bygone history of our flora and fauna, that we are glad to see that Mr. H. D. Geldart takes this view in his presidential address to the Norfolk Naturalists' Society. Comparing the existing flora of Greenland with the flora of Britain during the Glacial Epoch, he is no doubt right as to the power of part of the arctic and alpine flora to survive, however cold it may be. We may point out, however, that it can only survive if there still exist tracts of land bare of snow and ice during the summer. The British alpine species may have lived throughout the Glacial Epoch in the South of England or on the ice-free summits of our north country; they cannot well have lingered in the flatter districts of East Anglia, where the ice-sheet seems to have been continuous. Mr. Geldart thinks that the Greenland flora likewise inhabited Greenland through the Glacial Epoch; but he does not allow for the former greater thickness and extent of the ice, which then, in all probability, entirely hid the small areas now free from snow during the summer. The marks of glaciation in Greenland usually extend far beyond and above the present limits of the ice.

THE ANTARCTIC CONTINENT.

IN our issue of last month we dealt with recent views upon the former greater extension of the Antarctic Continent. A correspondent has pointed out to us the important evidence in favour of this view that is furnished by a study of the earthworms of the Southern Hemisphere. These animals are, generally speaking, entirely wedded to the soil; they are impatient of sea-water, and possess but few facilities for assisted migration; hence they are valuable as a help towards determining the probabilities of earlier land-connections. It is a striking fact that there is the closest similarity between the earthworms of Patagonia, New Zealand, and such of the intervening islands as have been explored. It appears from the papers containing descriptions of the earthworms of the "antarctic area" by Michaelsen, Rosa, and Beddard, that the bulk of the indigenous Oligochætous inhabitants of these regions of the world are members of the genera *Acanthodrilus* and *Microscolex*. Indeed, in Patagonia and the Falkland Islands no other species at all have been met with. In New Zealand, there are only two earthworms which are not of either of those two genera or which do not belong to the three genera *Octochætus*, *Deinodrilus*, and *Plagiochæta*, closely allied to *Acanthodrilus*. South Georgia, Kerguelen, Marion Island, and MacQuarie Island also possess two or three species of the genus *Acanthodrilus*. Only

six species of *Acanthodrilus* and something like the same number of *Microscolex* exist outside of this area. The iceberg theory of migration might be called in, perhaps, to explain the phenomena; but it offers great difficulties. It may be granted that the cocoons might be detached with a quantity of the earth in which they are usually deposited and find their way on to a glacier, and so, with its broken end, float out to sea. Plenty of dangers are run during these preliminary steps, but the real dangers only begin when the berg is well out to sea, and, while gradually diminishing in size, is exposed to the splashing of the waves of that stormy ocean. This would be almost certain to destroy some of the embryos contained within the cocoons. If these perils are escaped it can be but seldom; such rare occurrences would surely hardly account for the close similarity of the earthworm faunas that has been referred to. If the extension of the land be denied, some explanation is much wanted for these facts.

Distribution of Earthworms.

The California Academy of Sciences is to be congratulated, not only on the excellence of its publications in the matter of type and lithography, but on the good work done by its contributors. One of the latest numbers of the quarto memoirs of this Academy is before us, and deals with a large series of new species of earthworms from California, by Dr. Gustav Eisen. It is the second part of his "Pacific Coast Oligochæta." For some years past Dr. Eisen has been engaged in the study of the American Oligochæta, both fresh-water and terrestrial, of which there is a greater variety than in Europe. In this latest publication, Dr. Eisen has described five or six species of the Acanthodrilidæ, *Benhamia* and *Acanthodrilus*, and four new species of *Sparganophilus*. This latter genus was originally found in the Thames by Dr. Benham, but the occurrence of so many distinct forms in the North American continent seems to point to that area as its proper habitat. One species of the genus, of which, unfortunately, no other details are at present forthcoming, was found to possess eyes, at present a unique example among the earthworms. Eyes, as is known, occur in certain Naidæ, but no trace of them has as yet been met with in any terrestrial worm. *Sparganophilus*, however, though belonging to a family, Geoscolicidæ, which is chiefly terrestrial in habit, is itself aquatic, as are indeed a few other genera of the same family. The distribution of *Benhamia* is the most puzzling fact in the distribution of the entire order. Dr. Eisen, unfortunately, adds to the difficulties by describing what appear to be unquestionably indigenous forms from California. It is a genus which is eminently tropical in range, though a few (? imported) species have been found in extra-tropical regions, *e.g.*, in Germany. Dr. Michaelsen regards tropical Africa as its home, and in that continent are undoubtedly the largest number of species of the genus, which is always met with in

gatherings from Africa; but it also occurs, though to a less extent, through the East, and now we have a quantity of American forms. Dr. Michaelsen thinks that these species, which live out of Africa, have been introduced by commerce, and that they are not really indigenous elsewhere than in Africa; but there remains the fact that quite different species are inhabitants of America and the East. One of Dr. Eisen's new genera is also a member of the same family; he has named it *Aleodrilus*; it is mainly to be distinguished from other genera by the more posteriorly situated male generative pores. In this it agrees with the North American *Diplocardia*, of which further researches may show that it is only a distinct species. *Phœnicodrilus* is allied to a small worm, largely aquatic in habit, described by Beddard from tropical Africa, and named *Nannodrilus* on account of its pigmy proportions. Besides *Benhamia*, *Ocnerodrilus* is another genus which is common to the warmer parts of America and to tropical Africa. Resemblances between Africa and America have been pointed out in other groups of animals, and there is certainly some reason for comparing them in the matter of their earthworms; but it must be borne in mind that the common forms are small and of an accommodating way of life, so that accidental transference must not be lost sight of.

The Preliminary Notice.

Our Note on this subject (this vol., p. 73) has brought us a certain amount of correspondence. The principle of our criticism seems generally approved, though many workers are disposed to make reservations in one or another direction, and to

> "Compound for sins they are inclined to
> By damning those they have no mind to."

Mr. T. D. A. Cockerell is so bold as to send us "a little paper of his own" to serve as a test-case. Its very title is aggressive—"Preliminary Diagnoses of New Coccidæ." It is published in the *Supplement to Psyche*, February, 1896. "The writer," it begins, "having lately prepared descriptions of various new Coccidæ, which will appear in sundry bulletins, reports, proceedings of local societies, and so forth, it is deemed expedient to bring together some account of them here. This is done for the convenience of students, who sometimes complain of the difficulty of keeping track of scattered descriptions; and also to secure earlier publication, as some of the fuller accounts may be (indeed, have already been) much delayed." The diagnoses are short, but Mr. Cockerell assures us that they are "full enough for recognition," and that they are "actual abstracts of *full* diagnoses previously arranged for publication." The species come from Mexico, Japan, San Francisco, Trinidad, and elsewhere. Full particulars of most will be given in a forthcoming *Bulletin* of the U.S. Department of Agriculture. "The West Indian forms will probably receive full publication in Trinidad."

Mr. Cockerell asks for our criticism; he shall have it. And our remarks will be found applicable to many similar publications. We accept Mr. Cockerell at his own valuation, and say nothing as to the intelligibility or validity of his diagnoses. But, by the way, what is the difference between a diagnosis and a "*full* diagnosis"; in other words, how can one abstract a diagnosis? A diagnosis is, or should be, itself an abstract as condensed as is consistent with intelligibility. More than this cannot be diagnostic; and nothing that is diagnostic should be omitted even from a preliminary notice. However, this may pass as due to an incorrect use of language. The real question is as to the necessity for this little paper of Mr. Cockerell's. It is, he says, to obviate the difficulties arising from scattered publication. But there is no necessity, we presume, laid on this author to shower his coccids broadcast like carnival *confetti*. The desire for notoriety that leads some authors to publish ten papers where one would do, and to send them to all parts of the world for publication, is in itself reprehensible, but doubly so if it leads to the additional publication of one or more preliminary notices. We do not grasp the plea—"the convenience of students"; students ourselves, we are incessantly irritated, either by turning up preliminary notices that are devoid of detail and illustration, and without reference to the place where such may be found, or else, when we have relied for name and date on an illustrated and detailed account, by suddenly finding our synonymic tables upset owing to some obscure preliminary note that is not referred to in the completed paper.

We do not know whether Mr. Cockerell intends to give proper references to this little paper of his in the promised *Bulletin*, or whether he intends to re-introduce his species as n. spp. But instances of the latter form of annoyance are not rare: another correspondent points out that a new genus and species of holothurians, *Pelagothuria natatrix*, was introduced by Professor Ludwig in *Zoologischer Anzeiger*, xvi., p. 183, 1893, that a figure of it appeared in Lang's "Lehrbuch der vergleichenden Anatomie," August, 1894, yet that it was again published in Ludwig's Holothurioidea of the "Albatross," in October, 1894, as *nov. gen. et sp.*; he also complains that Dr. Hartlaub published a large paper on the Comatulidæ of the Indian Archipelago, in which a number of species were described as new, which turned out to have been described in a Vorläufige Mittheilung in the preceding year. If such be the treatment meted to it by its own authors, the preliminary notice would appear deprived of its last excuse, that of securing early or prior publication. No one will seriously contend that, amid the increasing press of publication and multiplication of unnecessary writings, scientific men should be encouraged in these vain repetitions and immature essays, whose chief use seems, after all, to be to enable their authors to receive criticism on the mistakes to which their hurried preparation naturally exposes them.

PRIORITY IN NOMENCLATURE, AND OUR PROPOSAL.

THE remarks on Zoological Nomenclature kindly sent to us by Mr. Cunningham present the views of the morphologist and practical naturalist, to whom changes in nomenclature are a needless nuisance, rather than of the systematist, to whom they are an unwelcome necessity. We admit that the Law of Priority can never, of itself, lead to finality, owing to the existence of a number of doubtful cases like those quoted by Mr. Cunningham. But his proposed substitute for the law, viz., "Choose what you consider the most correct description and classification," would lead to mere anarchy and chaos, for "tot homines, quot sententiæ" is still true in zoology. On the other hand, Mr. Cunningham's rule for the giving of new names—"no one should give a new name to a conception already named"—is merely the Law of Priority in other words. Obviously, some such law is a necessity, if we are to avoid the multiplication of synonyms or to have any attempt at a world-wide set of names. The difficulties in the application of the law are of two kinds. First, is it to be retrospective? and, if not, where is the line to be drawn, and who is to draw it? Secondly, when authors have published unintelligible or doubtful descriptions, who is to be the judge? These matters cannot be left to the individual caprice of naturalists, even so distinguished as Mr. Cunningham. It is for this reason that we made the proposal published in our last number. We say,—let the Law of Priority work! in most cases it will answer. Then let the doubtful cases be adjudicated on by specialists appointed *ad hoc*, and let their decision be accepted. Fixity of nomenclature of course is not anticipated, for that could be the result only of the stagnation of systematic zoology. Nevertheless, the acceptance of our proposal would do away with the changeableness that depends on mere whim, or on literature rather than on fact. We realise, indeed we have insisted, that the full carrying out of our ideas cannot be yet; the index to all published names must first be completed. But we are glad to learn, through communications with many zoologists, that our proposal is regarded with favour in various influential quarters. We shall be pleased to receive suggestions as to the best mode of carrying it out.

CAPE COLONY GEOLOGICAL COMMISSION.

IN our last November number (vol. vii., p. 366) we announced the appointment by the Government of Cape Colony of a Geological Commission, which is to report to the Secretary for Agriculture. The Commission has now appointed the following gentlemen to begin the work of surveying and mapping the country: geologist, G. S. Corstorphine, B.Sc. (Edin.), Ph.D. (Munich); assistant geologists, A. W. Rogers, B.A. (Cantab.), and E. H. L. Schwarz, A.R.C.S. The Commission also intends to publish in June a bibliography of South African geology, which has been compiled by Mr. Harry Saunders, the secretary

to the Commission. During the last ten years some £35,000 has been spent by the Government of Cape Colony for geological purposes; but complaints have been made that although science may have been advanced by the contribution of a scattered paper or two to English publications, or by the enrichment of the British Museum with a skeleton of *Pareiasaurus*, still the Colony itself has nothing tangible to show. For the present Commission an appropriation of £1,500 has been made for the months December, 1895—June, 1896. It is hoped that the future work of the Commission will be carried on by annual grants of £2,000. Although South Africa abounds in mining engineers, prospectors, and such-like practical geologists, of more or less competence, still not much advance in our purely scientific knowledge of its geology has been made since the days of A. G. Bain. The Commission intends to devote its energies purely to the scientific aspects of the science, and to steer as clear as possible of the ordinary speculator. By this means a secure foundation will be laid for the geology of Cape Colony. The Commission will be glad to receive copies of any geological publications, in return for which they offer to forward the reports on the geology of the Colony.

Moving Pictures.

The era of living pictures at the music halls is being replaced by an era of moving pictures. The living pictures, attractive though many of them might have been, were the antithesis of all art; pictures, in a sense, are studies from life; the living pictures were life posing as a study of itself, a truly *fin de siècle* conception. The moving pictures are wonderful reproductions of life on a screen. The spectators sit opposite what seems an ordinary magic lantern screen, and see thrown from the lantern, say, a country railway station. Gradually, before their eyes, a train steams up to the platform, passengers emerge, porters bustle about; the station gradually empties and the train is shunted out. The mechanism consists of an adaptation of series of consecutive, instantaneous photographs of moving objects. An account of the methods by which these may be taken is to be found in the treatises by Dr. Muybridge, and by Professor Marey (a translation of the latter's work having recently been issued by Mr. Heinemann, under the title "Movement"). The consecutive photographs are turned into lantern slides, and the novelty of the process consists in the adaptation of the movements of the lantern slides to the intervals of time that separated the actual events recorded on the different plates.

The Edinburgh Summer Meeting.

Although University Extension in general is on the wane, summer meetings in which instruction is tempered by excursions and amusements flourish exceedingly. We have received the programme

of the Edinburgh Summer Meeting, to be held at University Hall, Edinburgh, from August 3 to 29. Professor Patrick Geddes and Mr. Arthur Thomson, who were the founders of the Edinburgh meeting, continue to be its guiding spirits. There are two features about the course which seem to us to distinguish it honourably among similar meetings. First, there is an attempt made to teach the natural and social sciences alongside each other. It is possible to suggest criticism, both of the practicability of this object and of the methods by which it is proposed to attain it; but we have nothing but praise for the synthetic spirit of philosophy that has ruled the drafting of the courses. In less than a month it were foolish to attempt detailed and isolated courses on special subjects; the holiday learners require, and are able to obtain, instruction in the methods of observation and instruction in the value of observation; they are taught why to see as well as how to see. Secondly, the international character of the programme is most noteworthy. There are lecturers from Scotland, England, and Ireland; but, in addition, Professor Wenley is coming from the University of Michigan; Messrs. Paul Desjardins and Elisée Reclus are to represent French and Belgian thought, while Professor W. Rein, of the University of Jena, is to give a course upon the Life and Doctrine of Herbart. This delightful international character of a Scots seminary is a return to the old academic traditions of Scotland, which gradually died after England enjoyed the advantage of the union of the crowns.

ECONOMIC ENTOMOLOGY.

MISS E. A. ORMEROD'S "Report of Observations on Injurious Insects during the Year 1895" well maintains the reputation of that indefatigable worker, who, year by year, sends forth clear and accurate descriptions and life-histories of the insect-ravagers of crops and trees with summaries of the best methods for fighting them. The most important feature of the present report is the account of the two-winged flies injurious to horses, cattle, and sheep. Though the work is intended for the "practical man," the "pure naturalist" will not turn its pages without profit. He will find, for instance, confirmation of the old observation that certain of the carnivorous ground-beetles (Carabidæ) occasionally forsake their predaceous habits to devour greedily roots and fruits.

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I.

"Das Tierreich"

THIS proposed great work has been so much spoken about of late that a short account may be acceptable to our readers. The complete title is "Das Tierreich. Eine Zusammenstellung und Kennzeichnung der rezenten Tierformen. Herausgegeben von der Deutschen Zoologischen Gesellschaft." It is to deal with all the species of animals, now living or extinct within historic times, that have received a recognisable description. Besides giving co-ordinated diagnoses of these, it will mention all insufficiently described and doubtful species, as well as sub-species and varieties, and will describe any important developmental stages, alternating generations, and specially remarkable biological relations (presumably this means cases of parasitism, symbiosis, and commensalism). Under each species will be given its geographical distribution, the most important literature relating to it, and a complete list of synonyms, so that no name that has been used in the binomial nomenclature of living species will be omitted. Besides the diagnoses of species, families, and higher divisions, there will be provided systematic synopses and practical keys for the determination of species. The unavoidable length of the work will be reduced, so far as possible, by the use of abbreviations, of which a list will be given with each division. The employment of it will be facilitated by systematic and alphabetical indices for each division and each larger group, as they are published, and for the whole work when it is completed.

The general editor of the work is Geh. Professor Franz Eilhard Schulze, of Berlin, associated with whom is a committee composed of the President for the time being of the German Zoological Society, and Professor Dr. K. Moebius. The general editor is assisted by sub-editors, to each of whom is assigned one main division of the animal kingdom. So far the following sub-editors have been appointed: Professor F. Blochmann, of Rostock, for Brachiopoda; Professor O. Boettger, of Frankfurt a/M., for Reptilia and Amphibia; Professor F. Braun, of Königsberg, for Platyhelminthes; Professor O. Bütschli, of Heidelberg, for Protozoa; Professor C. Chun, of Breslau, for Cnidaria and Ctenophora; Professor F. Dahl, of Kiel, for Arachnoidea; Professor C. W. von Dalle Torre, of Innsbruck, for Hymenoptera; Professor L. Doederlein, of Strassburg, for Mammalia; Professor E. Ehlers, of Göttingen, for Bryozoa; Dr. W. Giesbrecht, of

Naples, for Crustacea; A. Handlirsch, of Vienna, for Rhynchota and Neuroptera; Dr. W. Kobelt, of Schwanheim, for Mollusca; Dr. H. J. Kolbe, of Berlin, for Coleoptera; Dr. H. Krauss, of Tübingen, for Orthoptera; Professor R. Latzel, of Klagenfurt, for Myriopoda; Professor J. Mik, of Vienna, for Diptera; Dr. G. Pfeffer, of Hamburg, for Pisces; Professor A. Reichenow, of Berlin, for Birds; Professor F. E. Schulze, of Berlin, for Porifera; Dr. A. Seitz, of Frankfurt a/M., for Lepidoptera; Professor J. W. Spengel, of Giessen, for Vermes, excluding Platyhelminthes, and for Tunicata. These subeditors will be assisted by specialists for the minor divisions, and among these there are announced the further names of Graf H. von Berlepsch, Professor R. Blanchard, Professor K. Brandt, G. Budde-Lund, Dr. O. Bürger, Professor G. Canestrini, Dr. E. Canu, Professor C. Emery, H. Friese, Professor A. Giard, Professor L. von Graff, E. Hartert, Pastor F. W. Konow, Professor K. Kraepelin, Professor P. Kramer, Dr. R. Lauterborn, Professor R. von Lendenfeld, Dr. H. Lenz, Dr. H. Lohmann, A. D. Michael, Dr. W. Michaelsen, Professor A. Nalepa, W. R. Ogilvie Grant, Dr. A. E. Ortmann, Dr. G. R. Piersig, Dr. L. Plate, Dr. R. Rhumbler, Dr. J. Richard, L. W. de Rothschild, Dr. F. Schaudinn, Dr. O. Schmeil, Dr. O. Schmiedeknecht, Dr. R. B. Sharpe, Rev. T. R. R. Stebbing, Dr. H. Uzel, Dr. W. Weltner, and Professor C. Zelinka.

To ensure that this array of contributors shall work on the same lines, a number of rules have been drawn up, which will no doubt be rigidly enforced by the editors. We have already commented upon the rules of nomenclature that are to be followed. For the denotation of colours, Saccardo's "Chromotaxia" is to be employed, except in such cases as more special terms may be advisable, *e.g.*, for the birds. The abbreviations of authors' names will follow that cryptographic production known as the Berlin Author-list. German is the language to be used, though English, French, or Latin may exceptionally be employed. German would be as good as any other language if only the Germans would use the Græco-latin technical terms that are used by all the other zoologists of the world; but if the contributors to Das Tierreich are going to continue to use ordinary German words, or, still worse, compound German words such as are given in no dictionary, for the technical terms of the science, then the adoption of this language will render the work far from popular among zoologists of other countries, and will materially detract from its value and its pecuniary success.

The size of the page to be employed is about 26 by 17 centim. (Lexicon 8vo). The paper has a rather smooth surface, is stated to be pure, and weighs eight sheets to the pound. The type is small pica for the diagnoses, and long primer (the type in which this article is set) for subsidiary information; it is set solid throughout. Hair-spacing, *italics*, and **clarendon** are used for distinctive purposes. Seeing that this work will inevitably run to an enormous size, and

that its chief use will be as a book of reference, we should urge, if it be not too late, the employment of a rather smaller type (say long primer and brevier), so as to get more into a page, which everyone knows is a great advantage in books of this nature. We should also have thought that a paper of rather less weight, such as that used for the Oxford Bibles or Liddell and Scott's Greek Lexicon, would have had many advantages over the rather stout and heavy paper adopted.

The work will appear in a series of Lieferungen, each dealing with one or more closely connected groups. The sizes of the Lieferungen will necessarily vary, but it is not intended that they shall be less than three sheets. It is estimated that the Platyhelminthes will occupy 4 Lieferungen, the Crustacea 11, the Hymenoptera 13, the Mollusca 15, the Reptilia 3, and the Aves 16. Each Lieferung will be sold separately. Those who promise to subscribe to all that are published during a period of five years will receive them at a price of about 70 pfennige per sheet. The price of separately sold parts will be calculated at one-third higher. Each part will be printed and published as soon as ready, without any relation to systematic order. Notwithstanding this, it is expected that it will take twenty-five years to complete the work. It is intended that the first part shall appear at the beginning of 1897, and in case enough subscribers are obtained, there will be issued an edition on writing paper, as well as one printed on only one side of the page. Orders for the whole work or for separate parts should be sent as soon as possible to the publishers, R. Friedländer & Sohn, 11, Carlstrasse, Berlin, N.W.

As a specimen-part, we have received "Heliozoa bearbeitet von Dr. Fritz Schaudinn," 24 pages, 1896. This begins with a systematic table of contents. Then follows a list of the six abbreviations used in this part for morphological terms; these are: Ectpl. *Ectoplasma*, Entpl. *Entoplasma*, Ncl. *Nucleus*, Pls. Vac. *Pulsierende Vacuole*, Psdp. *Pseudopodium*, Vac. *Vacuole*. The abbreviations for references to literature follow those given in the *Zoological Record*, and a list is given of such works as are here referred to but not included in the *Record*. The description of the division Heliozoa occupies nearly a page and a half, or a page and two-thirds if one includes the drop at the head of the first page, a feature that might well be dispensed with in a book where space is so extraordinarily valuable. In this space are to be found: first, authorities for the name *Heliozoa*; secondly, a diagnosis of six lines, hair-spaced; thirdly, a further explanatory description of twenty-one lines, just such as one might find in any text-book of zoology; fourthly, a description of the various diagnostic characters used in delimiting the minor divisions, accompanied by ten small figures of spicules, and occupying twenty-nine lines; fifthly, a line of small type devoted to geographical distribution; sixthly, the numbers of the orders, genera, and species, a statement that families are impracticable, and the rejection of the Vampyrellidæ from the group,

this all forms six lines of small type; lastly, a synopsis of the orders, in four lines of small type. Then follows the systematic treatment of the first order—Aphrothoraca, under which head are the authorities, a diagnosis, numbers of genera, synopsis, and key to genera. The genera of the order are then dealt with in similar manner, and lastly come the species, each with its references, synonyms, diagnosis, and distribution. The part concludes with an alphabetical index to all generic and specific names, synonyms being in italics.

The work appears to be clearly and carefully carried out, but one or two minor criticisms suggest themselves, mainly in the direction of the saving of space. Seeing that the abbreviations of morphological terms are explained under a boldly printed heading on p. 6, it was surely unnecessary to repeat them on p. 7. The word "in" could easily be omitted in such references as "Bory de St.-Vincent in: Encycl. méth., *v.* 2 p. 614"—a colon is quite enough. And surely t is unnecessary to give us the whole mouthful "Bory de St.-Vincent"; "B. de St.-Vincent," or "St.-Vincent" alone, is all that is needed, since the reference to Encycl. méth. prevents any possible ambiguity. Such an expression as "M. confluens Aim. Schneid. 1878 *M. c.*, Aimé Schneider," etc., also appears ridiculously lengthy. It will be noticed that Arabic figures only are used, and this necessitates the introduction of *v.* for volume, p. for page, t. for plate (Tafel), and f. for figure. All the abbreviations used are not explained, thus we find "Durchm.," "ca.," "L.," "Br.,"—of course these are readily intelligible to those accustomed to German systematic writing, but we are thinking of the others. The use of a | instead of a full-stop to separate clauses in the synonymic lists is to be commended. Some space is lost by the insertion of a rule and many leads between the orders; sufficient distinction would have been made by the use of small capitals for the ordinal names. It is a little odd that neither capitals nor small capitals are employed in any connection except as initials. It must, however, be admitted that the pages present a clear and pleasing appearance, and that the printing has been well executed by A. Hopfer.

Altogether the present part contains sixty species, both valid and doubtful. These, with their indices, but excluding the title-pages, occupy twenty pages—that is to say, three species go to a page. Now, since the number of recent species has been calculated at not less than 386,000, we may infer that "Das Tierreich" will occupy at least 128,700 pages. The length of the complete work, exclusive of the binding, will therefore be 12 metres 87 centimetres. The binding will bring it up to 13 metres, and this is no exaggerated estimate. Under these circumstances, we venture to think that every possible suggestion as to the means of reducing this enormous bulk is of value, and for this reason we have printed such suggestions as occur to us.

All success to Professor Schulze and his collaborators in their herculean task!

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II.

Change in Zoological Nomenclature.

IN the discussion of this subject at the recent meeting of the Zoological Society a number of eminent and experienced zoologists, including Sir William Flower, who presided, expressed dissatisfaction with the law of priority in the application of generic and specific names. They pointed out that, as a matter of experience, the uniformity in the use of names which the law is intended to produce has not been secured; and they did not believe that in this respect the experience of the future would differ from that of the past. It was urged by the President that, however clear and distinct the adopted rules of nomenclature appeared to be, different interpretations would be given to them by different minds. Several speakers mentioned the inconvenience which is frequently caused, and from which probably all working zoologists have suffered, by the disinterment and resuscitation of an old and forgotten name, used perhaps only once, to replace some well-known name which has been employed in several standard works. One speaker condemned such a proceeding as a scientific crime, but we all know that it is often committed in the sacred name of priority by some of the ablest and most learned naturalists.

Two instances, which have recently made a painful impression on myself, may be mentioned. The most eminent English ichthyologists have given the generic name *Rhombus* to the turbot and brill, and *Pleuronectes* to the plaice, dab, flounder, and other species. Dr. Günther has adopted this nomenclature in his British Museum Catalogue, which is, I believe, the latest survey of the species of fishes of the whole world. But two American ichthyologists lately published a Revision of the Flounders and Soles of America and Europe in which they named the genus of which the turbot is type, *Pleuronectes*. The argument of the American authors is that, although the name *Rhombus* was first used by Cuvier to distinguish the turbot and brill as a sub-division of Linnæus's great genus *Pleuronectes*, it was not declared by him to be a generic name, and that *Pleuronectes* was first applied as a generic name in a restricted sense in Fleming's British Animals, to distinguish turbot and brill from sole and plaice. This shows that the application of the law of priority is not so simple as it seems, and may easily lead to quibbles. Moreover, these changes often involve others.

Another instance is that of the lobster. It was called *Homarus vulgaris* by Milne Edwards; *Astacus* is the genus including the fresh-water crayfish, and *Gammarus* is a common genus of Amphipoda.

But, according to Mr. Stebbing's recent volume on Crustacea, the proper scientific name of the lobster, according to the law of priority, is *Astacus gammarus*, Leach.

The law of priority appears to be a dead failure: it does much harm and probably no good. Are there, then, any principles to guide us in the use of binomial designations, or are we to come to the conclusion that the binomial nomenclature is a failure?

One of the chief objects of scientific nomenclature is to express the mutual relations of living beings. Our classification aims at expressing the natural affinities of species so far as we know them; and nomenclature is the language of classification. It is mainly for this reason that Latin names are wanted at all. The names "horse" and "ass" imply no proposition concerning the degree of resemblance between the two forms, or their degree of difference from other forms; when we call them both *Equus*, we imply that they both exhibit certain characters which we define as generic. Since classification is always changing, nomenclature must change too; and since students do not agree concerning classification, they must also hold different opinions on nomenclature. Therefore, the hope of fixity or uniformity in nomenclature is necessarily fallacious. But the principle that follows from these considerations is that no change of names should be made except as an expression of new propositions in classification. But it will be said, What are we to do with synonyms?—when there are several names for one species, which are we to choose? The answer is, Choose that which is associated with what you consider the most correct description and classification. For example, *Homarus vulgaris* has been used by the latest and best authorities on the systematic affinities of the lobster, and therefore should not be changed. To say that this is an injustice to Leach seems to me a mistake. Leach's fame or historical importance has nothing whatever to do with the retention of the name he used; and no man's fame or importance can be allowed to stand in the way of scientific progress or of the convenience of the investigators and students of the present day. Let the dead bury their dead! The first consideration is the salvation of the living.

It seems to me it must be recognised that the name of a species has no definite and precise meaning unless accompanied by the name of its authority, either expressed or implied. This is another reason why the law of priority is futile and superfluous. If we write *Astacus gammarus*, Leach, or *Astacus gammarus*, Stebbing, we designate the lobster, and anyone who wishes to know what we mean can find out by reading the definition and classification of Leach or Stebbing; but if we write *Homarus vulgaris*, Milne Edwards, we also indicate the lobster, with the great advantage that the reader can find out much more easily and completely the characters and affinities of the species in question. It should be understood, therefore, that by the name of the authority is meant, not necessarily the author who first gave the

name, but the author whose diagnosis of the species is adopted by the writer quoting. In thousands of cases the name of a species without the authority for the name means nothing at all, because it may mean either of several species, or because there is no means of ascertaining what characters it implies. This only applies to names when simply quoted. If the name is given in a systematic revision with the diagnosis and description following, the name of the authority is of less importance, the writer is his own authority. There are many cases in which names are used without quoting the authority for them, because it is evident from the context, or it is understood, that the nomenclature of some standard work is followed. There are other cases, probably not very numerous, in which no authority is given, because the name is universally accepted, and any manual will supply the characters and position of the species; but, as a general rule, the name of an authority is necessary, and the most convenient practice is to give, not the name of the writer who first described the species, but of the writer who has made the most recent and accurate revision of its characters and position. In fact, in quoting, it does not matter what name we use, or whether we all use the same, provided that we use one by which the characters and affinities of the species designated can be ascertained with least trouble.

But up to the present only the citation of names has been considered; what are we to do about the giving of new names? It may be said that, if the first name has no more right than subsequent names, then there is nothing to restrain an author from giving his own names to species or genera already described, and so we shall have an endless multiplication of synonyms; but the reply to this is, that it should be a rule that no one should give a new name to a conception already named, and this rule would have as much force as the present rule of priority. The rule of priority does not prevent the describing of the same species under different names, and notoriously has not prevented the appearance of synonyms. If zoologists would be more careful to write diagnoses which were diagnostic, and descriptions which were intelligible, there would be less probability of the re-naming of species already described. There can be little doubt that the law of priority is a direct encouragement to careless and inadequate description, because workers are over-anxious to get their specific names published. If they knew that a name attached to a slipshod, obscure description was likely to be passed over and forgotten, and replaced by another given by a more careful worker, they would make less haste and more speed.

To sum up, the whole duty of systematists with regard to names is never to invent a new one or alter an accepted one, except to express a new conception. To resuscitate a forgotten name for a known form is as bad as to invent a new name for it.

J. T. CUNNINGHAM.

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III

A New Method in Soil-Analysis.

IT is proposed in the present paper to give a brief account of an important memoir on soil-analysis, recently contributed by Dr. Bernard Dyer to the Chemical Society, which would otherwise be likely to escape the notice of readers of NATURAL SCIENCE. As everybody knows, there are certain chemical elements, such as nitrogen, phosphorus, potash, calcium, and iron, whose presence in the soil is an essential condition of plant-life; and, naturally, one of the first results of modern chemistry was an analytical examination of the various soils in order to determine in what proportions these constituents of plant-food were present, and, on the other hand, what soils—whether owing to exhaustion by long-continued harvesting, or to natural poorness and infertility—required the addition of manures.

At first, no doubt, the problem appeared delightfully simple and straightforward. With the methods of modern analysis it was easy to make very accurate examinations of the soil; and if phosphoric acid, for instance, were found in minute quantities only, clearly that soil would require dressing with phosphatic manures; while if, say, ·20 per cent. of phosphoric acid were present, it would stand to reason that the soil was in a fertile condition and required no application of manures. Unfortunately, however, like many other things that "stand to reason," this simple inference was often sadly at fault. For instance, as Dr. Dyer points out, it is very usual to find about ·15 per cent. of phosphoric acid in an average English agricultural soil; and this represents about two tons of phosphoric acid an acre distributed in the upper nine inches of soil alone, to say nothing of the supplies in the sub-soil, which, of course, are also utilised by the plants: yet, strange though it may appear, the addition of a few hundredweights of phosphatic manure to this soil, which contains already as much phosphoric acid as ten tons of bone-dust or seventeen tons of superphosphate, will result in a full crop of turneps instead of a bad one.

In the light of such experience it was soon recognised that the determination of the *total* phosphoric acid present in the soil, and soluble in mineral acids, was of almost no value as an index to the fertility; since it was clear that only a small unknown proportion of that present was in a condition available for the use of plants; and that proportion, until recently, we had no means of determining,

although various attempts had been made to estimate it by extraction with acetic acid.

Passing over the long list of chemists, quoted by Dr. Dyer, who have worked at this difficult subject, and some of whom have more or less nearly anticipated him, we may point to the work of Stutzer as giving the key to the present inquiry. It had long been admitted that to determine the amount of available phosphates, in manures as in soils, digestion with strong mineral acids was useless; and the practice of extracting from manures with ammonium citrate had come largely into vogue, no doubt because the "precipitated" phosphates, which are known from experience to be largely available as a plant-food, had been found to be soluble in ammonium citrate. Against this practice Stutzer, however, strongly protested, and proposed instead the use of a 1 per cent. solution of citric acid as a solvent; and he showed that there was a considerable correspondence between the solubility of various phosphatic manures in citric acid and their availability for plant-food as tested by field-experiments. Unfortunately, however, Stutzer's solution was open to the objection that it was perfectly empirical; why should a 1 per cent. citric acid solution's dissolving power be an index to the amount of available plant-food? Here it is that Dr. Dyer steps in with a happy and original suggestion.

It had been known for long, in fact ever since Sachs' classical experiment of etching a marble slab by exposing it to growing roots, that the young roots excrete an acid liquid; and it was a fair inference that this acid excretion might be a means of bringing into solution otherwise unavailable constituents of the soil which would not dissolve in mere water. It therefore struck Dr. Dyer that, if one knew what percentage of organic acid was contained in this root-sap, it would be possible to place Stutzer's proposition on an altogether new footing. No data could be found in botanical works as to this acidity; and Dr. Dyer accordingly went to work to determine for himself the acidity of the root-sap in various plants. The work was by no means simple, and involved many difficulties of manipulation; but eventually a large collection of data was obtained. Since these data appear to be unique, and have a very wide scientific interest, it will be well worth while to extract the table entire. It will be unnecessary to trouble our readers with all the statistics collected by Dr. Dyer in his tables, and we will therefore be content with tabulating the acidity found in the roots in the moist (natural) state, (1) in terms of hydrogen, (2) in terms of crystallised citric acid. It will, of course, be understood that the acidity of the roots is not due solely, or necessarily even at all, to citric acid, but no doubt to a mixture of several acids; but for the practical purpose in hand it was convenient to calculate the determined acidity in terms of crystallised citric acid; the percentage in terms of hydrogen is, however, absolute, whatever be the acid. Thus, the formula of citric acid is $COOH, CH_2, COH, COOH, CH_2, COOH + H_2O$, molecular

weight = 210; there are clearly three atoms of acid hydrogen present, therefore three of hydrogen correspond to 210 parts by weight of citric acid. Similarly, one part of acid hydrogen corresponds to sixty of acetic acid CH_3, COOH, or three to 180.

PLANT.	Percentage of Acidity in Terms of Hydrogen.	Citric Acid.
RANUNCULACEÆ.		
Caltha sp. (Marsh Marigold)	·0116	·81
" " "	·0113	·79
CRUCIFERÆ.		
Brassicæ—White Turnep (half grown)	·0087	·61
" " "	·0083	·58
" " (very young)	·0061	·43
" " another year :—		
" " "Bulb" (1 in. diam.)	·0066	·46
" " (fine roots)	·0075	·52
Swede Turnep "Bulb" (3 in. diam.)	·0041	·29
" " (fine roots)	·0054	·38
Field Cabbage (thick roots)	·0047	·33
" " (fine roots)	·0084	·59
Garden Cabbage	·0100	·70
Brussels Sprouts	·0073	·51
" "	·0081	·57
Broccoli	·0103	·72
"	·0080	·56
Collards	·0064	·45
"	·0091	·64
Rape	·0053	·37
Cochlearia armoracia (Horse-radish)	·0126	·88
Alyssum sp.	·0140	·98
"	·0143	1·00
Cheiranthus cheiri (Wall-flower)	·0086	·60
" " " "	·0111	·78
Matthiola sp. (Stock)	·0084	·59
" "	·0084	·59
Lunaria biennis (Honesty)	·0120	·84
CARYOPHYLLACEÆ.		
Lychnis sp. (Scarlet)	·0096	·67
" "	·0135	·94
Dianthus barbatus (Sweet William)	·0083	·58
" " "	·0086	·60
TROPÆOLACEÆ.		
Tropæolum sp.	·0067	·47
LEGUMINOSÆ.		
Trifolium incarnatum (pot grown)	·0119	·83
T. repens (White Clover) (pot grown)	·0050	·35
" " " (field grown)	·0143	1·00
T. pratense (Red Clover) (pot grown)	·0065	·45
" " " (field grown)	·0157	1·10
Pisum sativum (Garden Pea) (pot grown)	·0043	·30
Vicia faba (Broad Bean) (pot grown)	·0072	·50
" " " " "	·0072	·50
" " " (field grown)	·0127	·89
ROSACEÆ.		
Geum sp.	·0527	3·69
"	·0429	3·00
Fragaria sp. (Strawberry)	·0219	1·53
"	·0217	1·52
ONAGRACEÆ.		
Œnothera biennis (Evening Primrose	·0220	1·54
" "	·0220	1·54
UMBELLIFERÆ.		
Pastinaca sativa (Parsnep)	·0072	·50
Daucus carota (Carrot)	·0040	·28
Petroselinum sativum (Parsley)	·0077	·54
" "	·0072	·50
Apium graveolens (Celery)	·0133	·93

Plant.	Percentage of Acidity in Terms of Hydrogen.	Citric Acid.
Araliaceæ.		
Hedera helix (Ivy)	·0098	·68
" "	·0069	·48
Dipsaceæ.		
Scabiosa sp. (Scabious)	·0052	·36
" "	·0050	·35
Compositæ.		
Helianthus tuberosus (J. Artichoke)	·0080	·56
" "	·0085	·59
Cichorium endivia (Endive)	·0062	·43
" "	·0053	·37
Campanulaceæ.		
Campanula sp. (Large Garden)	·0075	·52
" "	·0062	·43
Boraginaceæ.		
Myosotis sp. (Forget-me-not)	·0116	·81
Solanaceæ.		
Solanum tuberosum (Potato)	·0041	·29
Scrophulariaceæ.		
Veronica sp. (Speedwell)	·0119	·83
Digitalis purpurea Foxglove)	·0053	·37
" "	·0061	·43
Primulaceæ.		
Primula polyanthus	·0077	·54
"	·0069	·48
Plumbaginaceæ.		
Armeria vulgaris (Thrift)	·0230	1·61
" "	·0196	1·37
Chenopodiaceæ.		
Spinacea oleracea (Winter Spinach)	·0089	·62
Beta (Sugar Beet)	·0059	·41
" Red Chili Beet	·0071	·50
" Yellow Chili Beet	·0053	·37
" Red Mangold (pot grown)	·0083	·58
" Orange Tankard Mangold (pot grown)	·0076	·53
" " " " (field grown: bulb 2 ins.)	·0031	·22
" " " " " " fine roots	·0054	·38
Liliaceæ.		
Allium porrium (Leek)	·0027	·19
" " "	·0023	·16
A. cepa (Onion) "	·0059	·41
" " "	·0080	·56
Graminaceæ.		
Alopecurus pratensis (Fox-tail)	·0049	·34
Phleum pratense (Timothy) (pot grown)	·0071	·50
" " " (field grown)	·0073	·51
Poa pratensis (Smooth Meadow Grass)	·0037	·26
P. trivialis (Rough Meadow Grass)	·0069	·48
Dactylis glomerata (Cock's-foot)	·0069	·48
Cynosurus cristatus (Dog's-tail)	·0049	·34
Festuca heterophylla	·0076	·53
F. ovina (Sheep's Fescue)	·0040	·28
F. pratensis (Meadow Fescue)	·0079	·55
F. elaterii (Tall Fescue) (pot grown)	·0032	·22
Avena sativa (Oats)	·0061	·43
A. flavescens (Yellow Oat Grass)	·0084	·59
Hordeum distichum (Barley)	·0039	·27
Triticum (Wheat), "Rivetts"	·0041	·29
" " Red Square Head	·0049	·34
Lolium perenne (Perennnial Rye Grass (pot grown) ..	·0043	·30
" " " " (field grown ..	·0036	·25
L. Italicum (Italian Rye Grass (pot grown)	·0040	·28
" " " " (field grown)	·0059	·41

Anyone looking down these tables will be struck by the wonderfully close agreement between the distinct independent determinations of the same species in many cases—an agreement that speaks

volumes for the reliability of the data. In certain other cases, however, there is a wide and unfortunate divergence, mainly among the clovers; but since, in these cases, one duplicate was grown in a pot under the unhealthy conditions of a London roof, the small amount of acidity found is readily explained. We will not linger over these interesting tables, only drawing attention to the high acidity found in *Armeria*, *Œnothera*, and pre-eminently in the Rosaceæ —the last observation being one that may possibly be found hereafter to have important bearings upon fruit-culture.

Having obtained this mass of data, Dr. Dyer's next trouble was to decide what would be a fair general average to take as the strength of citric acid solution for his soil-extractions; and finally he decided to adopt Stutzer's original proposal of a 1 per cent. solution as a fair approximation to natural conditions. It was objected, in the discussion which followed the reading of his paper, that this was too strong a solution, since, according to his tables, the average acidity of the chief agricultural plants, viz., Cruciferæ, Leguminosæ, and Graminaceæ, was about $\frac{1}{2}$ per cent.—that of the Graminaceæ being almost without exception below $\frac{1}{2}$ per cent. There was much apparent reason in this objection; but it was almost met by experiments that Dr. Dyer had made on the solvent powers of various strengths of solution on the soils: the following figures, showing the percentage of phosphoric acid dissolved from the soil by citric acid solutions of various strengths, will illustrate our meaning:—

·125	per cent.	citric	acid	solution	dissolved	·0200	P_2O_5
·25	,,	,,	,,	,,	,,	·0250	,,
·5	,,	,,	,,	,,	,,	·0264	,,
1·0	,,	,,	,,	,,	,,	·0312	,,
2·5	,,	,,	,,	,,	,,	·0728	,,
5·0	,,	,,	,,	,,	,,	·0896	,,

It is obvious that the difference between the solvent powers of a ·5 and a 1·0 per cent. solution is small; and this fact much diminishes the weight of the objection. Moreover, any agricultural analyst, with Dr. Dyer's data before him, could adapt his solutions so as to correspond exactly with the acidity of the crop which it was proposed to grow on any particular soil.

We must now proceed very briefly to indicate the happy verification of these conclusions that Dr. Dyer was able to obtain. Everybody has heard of the Rothamstead experiments, and knows that for over fifty years various plots of land there have been treated in every imaginable way as regards manures and crops, and complete histories of every plot preserved. Samples of these soils were placed at Dr. Dyer's disposal by Sir J. Lawes; and their analytical examination by the new method conclusively showed that the amount of phosphoric acid dissolved by a 1 per cent. citric acid solution was a fair index to the amount of available phosphoric acid and potash contained in the soil. It is somewhat difficult to exhibit in its cogency the evidence for this conclusion without going far beyond the limits of our

space; but we will endeavour to put the matter clearly before our readers in a few words. Certain plots have been manured completely, and parallel plots in exactly the same way but with no phosphates. The latter contain 2,730 lbs. per acre of P_2O_5, and the former 4,544—a difference of less than 1 : 2; but the amounts of P_2O_5 soluble in citric acid are respectively 237 lbs. and 1,259 lbs.—a difference of nearly 1 : 6. From these facts Dr. Dyer infers that the amount of available plant-food is nearly six times as great in the second case as in the first; and turning to the Rothamstead records we find that the respective yields of barley in 1890 were $38\frac{5}{8}$ and $24\frac{3}{8}$ bushels an acre—a grand difference truly! Yet the determination by the old style of merely the *total* phosphoric acid present would have told us absolutely nothing; for, as Dr. Dyer says, "the difference between 2,500 lbs. of total P_2O_5, and 4,500 lbs., per acre appears to be immaterial as a measure of present fertility for a crop that requires *only* 20 *lbs.* per annum for its actual sustenance"! This single example will suffice to explain to our readers the revolution effected by the citric acid test in soil-analysis—one of the most important factors in that scientific farming by which alone we can expect to get the full benefit of Mother Earth's capabilities. We will not yield to the temptation to multiply technical examples nor to adduce the parallel evidence from the estimations of potash in the soils—contenting ourselves with only quoting Dr. Dyer's guarded opinion that soils containing less than ·01 per cent. of P_2O_5, or, perhaps, ·005 per cent. of K_2O, soluble in a 1 per cent. citric acid solution, stand in immediate need of phosphatic or potash manures.

To sum up the *differentia* of Dr. Dyer's work. He has applied Stutzer's process of analysing manures to the analysis of soils, and has placed this purely empirical method upon a sound botanical basis; he has then verified it by a laborious analysis of numerous Rothamstead soils, the history of the crops from which entirely bears out the inferences drawn from his analytical examinations. We may add that Dr. Dyer has since employed his new method in investigating the available phosphoric acid and potash in the Rothamstead wheat-soils, and expects shortly to publish his results, which are awaited with much interest.

F. H. Perry Coste.

IV.

Museum Work in Jamaica.

NOTWITHSTANDING all the criticisms which have appeared, from time to time, in NATURAL SCIENCE upon the aims and objects of museums, there are very few which throw much light upon the claims which a museum in any one of our smaller British Colonies has upon its curator.

In a provincial museum at home he is advised to make local collections and illustrations his main object, leaving for those of the metropolis and a few of the largest cities the additional responsibility of attempting to represent the more complete series of the world's typical animal and vegetable forms. Here, however, one has the duty of the provincial with the responsibility of the national to combine. So far as practical acquaintance with nature is concerned, Jamaica, to the majority of its inhabitants, is the universe, hence to represent in its museum only objects of local importance is to insulate the already inexperienced mind still further. Undoubtedly, in the cause of science, the first object of such a curator should be to obtain the fullest representation possible of the local fauna and flora, and in the next place to endeavour to supply foreign forms for purposes of more complete exemplification and instruction.

Again, in most institutions it is considered sufficient for a curator to confine himself to the acquisition, arrangement, and study of specimens; leaving the teaching of their lessons, the stimulation of the interest of the public and the student in natural history, and the guidance of the original worker, to the professors and staff of some teaching organisation. Here, however, to the duties of one's appointment are added, "the delivery of lectures, the stimulation of original scientific research, and the taking of such steps as may be necessary to best advance the scientific interests of the island." When, added to this, he is the main recognised authority upon the applications of science to economical questions, the qualifications necessary to the training of a curator for a colonial museum evidently require much consideration.

The Museum in Jamaica is one of the components of the Institute of Jamaica, an organisation existing for the advancement of Literature, Science, and Art in the island; and embracing also a well-established Public Library and Reading Room and an

embryonic Art Gallery. It is managed by a Board of Governors, and practically the whole support is derived from the Legislature. Members are elected with certain privileges, and Members' Meetings are held. A journal devoted to the special objects of the Institute is published at intervals. Courses of public lectures on science and literature, on the lines of the University Extension courses in England, are arranged from time to time.

Up to the present the literary side of the Institute has flourished, to the disadvantage of the scientific. Now, however, vigorous steps are being taken to secure full recognition of the latter. On November 27, 1895, a new Museum, more adapted to the special requirements of suitably exhibiting objects, was opened under favourable circumstances. In view of some important anthropological discoveries recently made in the island, connected with the aboriginal Indian inhabitants, advantage was taken of the opening of the new Museum to stimulate further interest and researches in the subject. An anthropological exhibition devoted to Arawak remains was organised, and has proved a great success in encouraging investigations of the numerous caves and various deposits which are to be found in the island. Several series of aboriginal crania and other bones have been obtained; numerous examples of pottery, chipped flint and other stone implements, images, perforated shell ornaments, rock carvings, and several series of shell-mounds or kitchen middens have been brought to light; when their investigation, now in progress, is complete, these will form a very important contribution to the previously neglected anthropology of Jamaica. During the special exhibition, public demonstrations are being given upon the collections, especially to the teachers in the training colleges, with a view to direct and encourage research throughout the island.

Placed in the Museum, and arranged according to the parishes into which Jamaica is divided, are the valuable collections made by the officers of the Government Geological Survey under J. G. Sawkins, F.G.S., in 1869, with the corresponding geological maps and sections placed above the cases.

The palæontology of Jamaica was partially worked out by Mr. R. Etheridge, F.R.S., when he was Palæontologist of the British Survey; while the late Dr. Duncan has done much upon the fossil corals, and Mr. Guppy upon the shells. Professor Grenville A. J. Cole, Dublin, is at present engaged upon specimens of Jamaican rocks. With this exception practically no geological work has been done since the time of the survey.

The representatives of the fauna and flora of Jamaica are presented in very variable conditions of completeness. While the birds, reptiles, fishes, molluscs, and insects are well represented, other groups, especially the marine invertebrates, are sadly deficient.

The collection of birds contains many interesting specimens, including the three characteristic species of humming-birds. Jamaican

birds have been collected and studied more specially by Mr. P. H. Gosse (1847) and Sir Edward Newton. The collection formed by the latter, while Governor of the island, is in the museum at Cambridge, England.

Jamaica possesses only one indigenous land mammal, *Capromys brachyurus*, Hill, with the exception of the bats, of which there is a rich variety. Four species of snakes, twenty-two species of lizards, and several species of tortoises and turtles occur, while the crocodile is not uncommon. Over three hundred species of fish are known. A few live specimens of the most suitable animals are also exhibited in the grounds of the Institute.

A Herbarium of Jamaican plants, formed by Mr. Fawcett, the Director of the Botanical Gardens, is maintained; as also a duplicate of the Jamaica Court at the Imperial Institute, London, for purposes of reference.

The introduction of the mongoose into the island, about twenty years ago, for the purpose of destroying the sugar-cane rat, has very seriously disturbed the previous balance of nature among the various land animals and plants.

In almost every department of biological enquiry Jamaica and the West Indies generally offer a very rich, but only partially investigated, field for research. A vigorous attempt was made a few years ago to form a marine laboratory upon a large scale, with the special object of affording facilities to foreign biologists in studying tropical life, but unfortunately the scheme fell through, largely because of its too ambitious nature.

A biological laboratory with most modern appliances for carrying on scientific research, and a dark room for photography, have lately been fitted up in connection with the Museum. What with constant and regular intercourse with the outer world by means of various lines of steamers, a library furnished with most of the European and American publications, a laboratory, constant communion with professional men trained in England and America, one feels none of the insulation which might be imagined to exist in such a distant colony; while the varied richness and luxuriance of the tropical land and waters add a completeness to the biologist's conception of life not otherwise attainable.

J. E. DUERDEN.

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{591.16

V.

Reproductive Selection.[1]

(1) In a recent memoir ("Contributions to the Mathematical Theory of Evolution, III. Regression, Heredity, and Panmixia," now in type for the *Philosophical Transactions*) I have found it necessary to note the difference in mean and variation of a population when (*a*) the individuals of a sex are taken into account *once* as mates, (*b*) when the individuals of a sex are treated as parents or weighted with their fertility. The mean and variation of the population are supposed to be taken with regard to any organ whatever. If such a difference is found to exist between the variation curves for mates and for parents, then there is a correlation between fertility and the organ (or characteristic) measured. Under the action of heredity there will accordingly be a progressive evolution in this organ, unless this evolution be checked by some other factor of progressive change, *e.g.*, natural selection. In my memoir I term this factor of progressive evolution *Reproductive Selection*.[2] Without wishing at present to publish my complete work on this subject, I should like to put on record the following conclusions already reached:—

(2) Let any organ in individuals of one sex be selected, and let y be the fertility of an individual, whose organ differs x from the mean organ of all mated individuals. Let M_m be the mean organ for all mates,[3] M_p be the mean organ for all parents, *i.e.*, a mate reckoned

[1] A Contribution to the Mathematical Theory of Evolution, read before the Royal Society on March 5.

[2] The influence of variation in fertility has been considered by Mr. Romanes under the title of 'Physiological Selection,' but the idea he expresses by this term appears to me very different from that of reproductive selection. In mathematical language, Mr. Romanes supposes the fertility curve and the correlation surfaces, owing to some cause or other, to become double-humped; they may accordingly be resolved into two components, each corresponding to a distinct species. Physiological selection thus aims at an explanation of the origin of species. Reproductive selection supposes the fertility curve and correlation surfaces to embrace only homogeneous material, and it can accordingly never give rise to a new species; it is purely a source of progressive change in the same species. The only approach to a double hump which occurs in the curves of human fertility that I have dealt with is a secondary maximum at absolute infertility, due in all probability to artificial restraint on fertility. As those couples who fall into this component leave no offspring, they cannot give rise to a new species.

[3] If there be preferential mating, M will not be the mean organ for all individuals. I have adopted the mate mean in order to free the investigations from the influence of this portion of sexual selection.

once for each offspring. Let M_0 be the mean of the offspring for the same or any other organ, taking one or any other number equally from each mated individual; let M_1 be the mean of all offspring. Let σ_m, σ_p, σ_0, σ_1 be the corresponding standard deviations, reckoned from the formula: σ^2 = (sum of squares of deviations) ÷ (number of individuals), and without regard to any special law of variation, such as Laplace's law of errors.

Let r_0 be the coefficient of correlation between parent and offspring, each parent being given only one or, at any rate, an equal number of offspring, *i.e.*, r_0 is the coefficient of pure heredity for the organs in question, supposing fertility to be uniform, or at any rate to have no correlation with the organ or characteristic under investigation. Let ρ be the correlation between fertility and the given organ in the parent, and let v equal the coefficient of variation of fertility in the parent, *i.e.*, if y_m be the mean fertility: $v = \sigma_f/y_m$, where σ_f is the standard deviation of parental fertilities. Let $y' = y - y_m$ be the deviation from mean fertility of the parent with organ x. The values of r_0 and ρ are to be calculated from the formulæ—

$$r_0 = \frac{\text{Sum of (deviation of offspring} \times \text{deviation of parent)}}{\text{Number of pairs of offspring and parent} \times \sigma_0 \times \sigma_m},$$

$$\rho = \frac{\text{Sum of (deviation of mate} \times \text{deviation of mate's fertility)}}{\text{Number of mated pairs} \times \sigma_m \times \sigma_f},$$

where, in r_0, each parent is to be taken only once, or at any rate the same number of times.

Thus r_0 and ρ are absolutely independent of any special distribution of variation.

Then the following results hold if n be the number of mated pairs:—

$$M_p = M_m + \rho v \sigma_m \quad \text{(i)}.$$

$$\sigma^2{}_p = \sigma^2{}_m (1 - \rho^2 v^2) + \frac{S(x^2 y')}{n y_m} \quad \text{(ii)}.$$

$$M_1 = M_0 + r_0 \rho v \sigma_0 \quad \text{(iii)}.$$

$$\sigma_1{}^2 = \sigma_0{}^2 \left(1 - r_0{}^2 + r_0{}^2 \frac{\sigma_p{}^2}{\sigma_m{}^2}\right) \quad \text{(iv)}.$$

The first three equations are true whatever be the distribution of variation in mates, parents, offspring, and fertility; the fourth equation assumes the standard-deviation of a fraternity or an array of offspring to be $\sigma_0{}^2 (1 - r_0{}^2)$. This result would flow for normal correlation between organs in parent and offspring, a type of correlation which holds closely for inheritance in the case of man. It would also flow from *any* law of variation which gave a constant coefficient of regression and a constant standard deviation for the array. What, however, is the important point is this, that no assumption has been made with regard to the nature of the fertility correlation. This is essential, as in the case of man this correlation is certainly, like the distribution of variation in fertility, markedly skew and not normal in character. Our equations accordingly amply cover facts, which they

could not cover had they been based solely on the usual or normal theory of correlation.

(3) By simply forming the means for any organ (or characteristic) for mates and for parents, we can ascertain from Equation (i) if there is or is not any sensible correlation between that organ (or characteristic) and fertility. Equation (ii) enables us to verify the value found for ρ, since σ_p and σ_m are easily calculated when we know the distribution of fertility. If the correlation were normal $S(x^2y')$ would be zero, and this term, it may reasonably be expected, will never be very large. When ρ has been found from Equation (i), then Equations (iii) and (iv) give us $M_1 - M_0$ and $\sigma_1 - \sigma_0$, or the measures of reproductive selection in its action on the mean and variation of successive generations.

(4) I have applied these results to the only case—that of man—in which statistics are at present available.

I find for upwards of 4,000 families, principally of Anglo-Saxon race, $v = 0{\cdot}692$, and for 1,842 families of Danish race, $v = 0{\cdot}652$. This, considering difference of race, is a very satisfactory agreement. In the next place, there appears to be a significant difference $0{\cdot}278''$ between the mean height of mothers of daughters and the mean height of wives. Thus, we have $\rho v \sigma_m = 0{\cdot}278''$, and since $\sigma_m = 2{\cdot}303''$, it follows that $\rho v = 0{\cdot}121$. Now, the coefficient of variation for fertility in daughters is not quite the same, but still very nearly the same as that for fertility in general. We therefore find that $\rho = 0{\cdot}175$ to $0{\cdot}186$, according as we use the first or second value of v given above. We may accordingly conclude that there is a sensible correlation (*circa* $0{\cdot}18$) between fertility and height in the mothers of daughters.

Turning now to Equations (iii) and (iv), I note that r_0, σ_0, and r_0^2 are multiplied by the small quantities ρ and $1-(\sigma_p/\sigma_m)^2$, and that r_0 and σ_m only differ from r_p and σ_p by quantities of the order ρ. Hence, neglected to a first approximation ρ^2, we can use the value r_p, already known, for r_0 in (iii) and (iv) and the value σ_1, already known for σ_0 in (iii), we thus deduce—

$$M_1 - M_0 = 0{\cdot}081''.$$
$$\sigma_1 - \sigma_0 = -0{\cdot}008''.$$

These are the effects of reproductive selection on the height of women. We thus see that the effect is to render women less variable, and to raise their mean height. The quantities are very small, but it must be remembered that the process is secular. Thus, supposing reproductive selection to have been unchecked by natural selection, say, for forty generations, the mean height of women, neglecting small quantities of the second order, would have been raised about $3\frac{1}{4}$ inches. A factor which would alter stature by about 3 inches in 1,000 years is clearly capable of producing very considerable results in the long periods during which evolution may be supposed to have been at work. In the case of both mean and standard deviation the

changes from wives to daughters (0·25″ and 0·044″) are, in the only statistics at present available, far more considerable than the above values; but it must be remembered that other causes than reproductive selection are at work, such as shrinkage with age and the greater physical training of the young women of to-day.

(5) I have only been able to measure, so far, the actual value of the correlation between fertility and any organ in the case of stature in women. It would, doubtless, be more sensible in other cases, *e.g.*, pelvic measurements. But there are certain considerations which may be referred to here, and which will suggest how important—at any rate in the case of man—it is to take into consideration the influence of reproductive selection.

From considering the fertility of man, in England and in Denmark, I conclude that 25 per cent. of the mated population produce one-half the next generation. This is the gross fertility. Allowing for the selective death-rate—which I knew only for Denmark—27 per cent. of the mated population produced half the next generation. In other words, although natural selection tends to counteract reproductive selection by a death-rate which, it may be shown, rises continuously and uniformly with increased fertility, yet, in the case of civilised man, it is totally ineffectual as against reproductive selection. If we allow for the portion of the population which remains unmarried, we are well within the mark if we say that less than 25 per cent. of one generation produce more than half of the next generation. Correlation, therefore, between fertility and any mental or physical characteristic must work a progressive change.

We know that there are very considerable race and class differences in the matter of fertility. It is very difficult to understand how these could have arisen by the action of natural selection combined with heredity, unless either (1) fertility be inherited, or (2) fertility and some inherited mental or physical characteristic be correlated. But either (1) or (2) involves reproductive selection. We have seen that there is evidence of correlation between the stature of women and their fertility. There is also evidence of a correlation between fertility and class. Taking Copenhagen, for which alone we have satisfactory class-fertility statistics, it is possible to show:—

(i) That the gross fertility of the artisan is more than the gross fertility of the professional classes.

(ii) That the net fertility of the artisan is less than the net fertility of the professional classes.

Thus natural selection, at first sight, checks reproductive selection, greater fertility connoting a greater death rate; but we find:—

(iii) That the marriage rate of the artisan is so much higher than the marriage rate of the professional classes that the percentage fertility of the former considerably exceeds that of the latter.

Thus, while a selective death rate checks reproductive selection as between class and class, a selective marriage rate, again, places reproductive selection at an advantage as compared with natural selection; the population would accordingly appear to be ultimately, and in the long run, reproducing itself from the artisan classes.

I hope, later, to publish the analysis, curves, and statistics on which these conclusions are based; at present I only wish to draw attention to the general result: that reproductive selection—at any rate in civilised man—seems a factor of evolution equipotent to natural selection, if, indeed, it be not prepotent.

KARL PEARSON.

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VI.

Lyell and Lamarckism.

A Reply to Professor W. K. Brooks.

PROFESSOR BROOKS, in Natural Science for February, p. 89, has informed us that Romanes was mistaken in including him among American Lamarckians, and has at the same time explained why he considers Lamarckism untenable. His reasons do not appear to me adequate, and although I can scarcely hope to induce him to alter his intellectual position, I desire to ask the attention of other biologists to the following criticism of his arguments.

He believes, apparently, that his objections are essentially those expressed by Lyell in his "Principles of Geology," published long before Darwin's theory was made known. It is true that Lyell was unconvinced by Lamarck, but convinced of the truth of evolution by Darwin. Brooks maintains that Lyell's objections to Lamarck's particular views still remain unanswered, and supposes, if I understand him rightly, that his article merely recapitulates and expounds Lyell's arguments. Careful consideration of the article leads me to conclude, first, that Brooks's arguments are not those of Lyell, and, secondly, that they are not sound. I will give my reasons for these conclusions in their order.

According to Professor Brooks, Lyell proved that the effects of the environment were not inherently adaptive, and therefore, whether inherited or not, could not be accepted as the cause of the evolution of adaptations. After perusal of Lyell's reply to Lamarck's views in the "Principles of Geology," I fail to find any indication of this proposition. The general tenor of Lyell's argument, as I understand it, is that the environmental forces may and do produce results some of which are adaptive, and which do become hereditary, but that the departure from the specific type so caused is definitely limited; and that the capacity for change within these limits is merely a fixed property of the species, which, like other specific properties, is immutable in its extent. The importance of the question is sufficient justification for quoting in full Lyell's own summary of his arguments. It is as follows:—

1. "There is a capacity in all species to accommodate themselves, to a certain extent, to a change of external circumstances, this extent varying greatly, according to the species.

2. "When the change of situation which they can endure is great, it is usually attended by some modifications of the form, colour, size, structure, or other particulars; but the mutations thus superinduced are governed by constant laws, and the capability of so varying forms part of the permanent specific character.

3. "Some acquired peculiarities of form, structure, and instinct, are transmissible to the offspring; but these consist of such qualities and attributes only as are intimately related to the natural wants and propensities of the species.

4. "The entire variation from the original type, which any given kind of change can produce, may usually be effected in a brief period of time, after which no further deviation can be obtained by continuing to alter the circumstances, though ever so gradually; indefinite divergence, either in the way of improvement or deterioration, being prevented, and the least possible excess beyond the defined limits being fatal to the existence of the individual.

5. "The intermixture of distinct species is guarded against by the aversion of the individuals composing them to sexual union, or by the sterility of the mule offspring. It does not appear that true hybrid races have ever been perpetuated for several generations, even by the assistance of man; for the cases usually cited relate to the crossing of mules with individuals of pure species, and not to the intermixture of hybrid with hybrid.

6. "From the above considerations it appears that species have a real existence in nature; and that each was endowed, at the time of its creation, with the attributes and organisation by which it is now distinguished."

I think there can be no dispute, with this summary before us, that Lyell's objection to Lamarck's views was, not that the effects of the environment were non-adaptive, but that they were permanently limited in extent. The doctrine of the fixity and essential immutability of species having been abandoned, as it ultimately was by Lyell, it follows that, in the judgment of both Lyell and Darwin, there was no ultimate limit to the extent of adaptive and hereditary modification that could be produced directly by changes in the environment.

Professor Brooks's argument having been shown not to have the authority of Lyell, which he claims for them, we will proceed to consider them on their own merits. He takes the case of the strengthening of muscles by exercise, which he admits to be an adaptive, *i.e.*, a beneficial, change in relation to the conditions calling it forth, "one of the simplest examples of the beneficial effects of the conditions of life." But this, he tells us, is not an example of the direct action of the environment on the organism. On the contrary, the increase and improvement of the muscle are due to the increased supply of nourishment produced by exercise; and the fact that exercise causes an increased circulation of blood in the muscle depends upon structural adjustments, which themselves constitute an adaptation, originally, I suppose, due to congenital variation and selection. Now I think it may well be held that the supply of nourishment is not the same thing as assimilation and growth; and the assimilation and growth of the muscle under stimulus must be

ascribed to a fundamental property of protoplasm, which it is not the business of Lamarckians or evolutionists of any other school to explain. But, apart from this, let us consider how far back in the phylogeny of vertebrates we must go to find the origin of a circulatory system; and consider that, from this point onwards, if changes in the locomotor system are due to the use and disuse of various parts of this system, and are inherited, then there is practically no limit to the variety and perfection of the adaptations in the locomotor system which can be explained by the Lamarckian factor. If selection produced in the remote ancestor the circulation and the structural arrangement by which muscle (and, of course, bone and other connective tissues and nerves) received increased supplies of blood when exercised, then it would require to produce nothing else to account for such adaptations as the neck of the giraffe, the various arrangements of the toes and limbs in ungulates, the peculiarities of the legs and skeleton in man which enable him to maintain the erect attitude, the form of the pectoral muscles and sternum in flying birds, the absence of the keel on the sternum in ostriches, the peculiarities of the tongue and hyoid bone in woodpeckers, of the beak in crossbills, and thousands of other mechanisms, all consisting in adjustments of muscle, bone, and nerve.

Professor Brooks next proceeds to argue that organs are only improved by normal or natural use, and that only with this limitation is use beneficial. "The ways to use a muscle are few, while the ways to abuse it are innumerable, and the inheritance of *all* the effects of the conditions of life must lead, not to 'cumulative adaptation,' but to cumulative destruction"; but no Lamarckian supposes that the action of the environment is necessarily beneficial. It would be interesting to know what meaning Professor Brooks attaches to the words "the ways to abuse a muscle are innumerable." To myself they are unintelligible. A muscle can be used, disused, or over-exerted—in fact, there is only one way to use it, and that is to contract it. Excessive strain which does not cause development and increase of the muscle must lead to injury and ultimately to the death of the organism. Disuse leads to degeneration. Degeneration is not necessarily beneficial, but it is included in the conception of adaptation, and it may be beneficial in relation to the adaptation of the whole organism to a new mode of life. The case of a herd of antelopes hunted by carnivores will illustrate the different points of view of the Lamarckian and the selectionist. Certain individuals, we may suppose, are caught and killed, others may escape and die of fatigue or an over-strained heart, while the rest escape and recover their strength. The fact that some are killed is in no way inconsistent with the fact that the survivors are improved with respect to the muscles and bones on which their running powers depend. It may even be that the persecution is so great that the constitutions of the whole herd are injured, their progeny are not sound, or are

weaker, and the result is extermination; but the inheritance of *all* the effects of the conditions of life does not always lead to cumulative destruction; if it did, there would be nothing left alive, and the statement that on Lamarckian principles it would, is equivalent to saying that the conditions unfavourable to life are, on the whole, more powerful than those which are favourable, a statement we know to be untrue. What Lamarckians contend is, that the conditions of life kill a large number of individuals and modify those which survive; that the struggle for existence not only involves the survival of the fittest, but the formation of the fittest. Professor Brooks's argument shows that he has quite failed to understand the Lamarckian view, and suggests strongly that he has never tried to do so.

Professor Brooks maintains that the structural changes associated with mental development are also, like the modification of muscles, dependent on structural adjustments for bringing about this development, and that the effects of the environment which are not "already deducible from adaptive structure, must be haphazard." But we are not told what are these other effects of the environment. The effects which are stated by Brooks to be dependent on pre-existing capacities are those which Lamarckians have chiefly in view. The others, it is said, must be haphazard because they are not proved to be beneficial. Now, to take an example, the excessive consumption of alcohol is not beneficial, but are its effects therefore haphazard? It does not follow that, because the effect of a given condition is not beneficial, therefore it must be indeterminate. On the contrary, science is founded on the conclusion, drawn from experience, that the same conditions always produce the same effects, and that is all that Lamarckians contend for.

We see, then, that Brooks's objection to Lamarckism, so far as the effects of use and disuse are concerned, is that such effects are due to structural arrangements already existing for the local increase or decrease in the supply of the nutritive fluids, or blood, and that Lamarckism does not explain these structural arrangements. We have seen that, if this objection were valid, Lamarckian effects would explain all the modifications in animals which have taken place since they had a circulation. But we have seen also that the objection is fallacious, because the supply of nourishment does not necessarily imply assimilation and growth, which are the results of stimulation. The effects of a profusion of nourishment and absence of stimulation are sufficiently notorious in the phenomena of parasitism. Is Professor Brooks unaware that Lamarckism extends to plants, and if not, can he suggest that the modification of flowers by the irritation due to insects depends on structural arrangements for the supply of nutrient fluids?

Professor Brooks has misinterpreted in the most astonishing manner one statement of Romanes, that "no question of value, as selective or otherwise, can obtain" in the case of Lamarckian factors.

He cites this as an admission on the part of Romanes "that it is, to say the least, no better than an even chance whether their influence be good, bad, or indifferent." What Romanes means is that it does not matter how slight the adaptive effects of the conditions may be; if they are inherited at all, they will be present in all the individuals exposed to the conditions, and will be increased in every generation; whereas before a variation can be selected, it must be supposed to have 'selection value'—that is, must be large enough and important enough to give a better chance of life and reproduction to its possessor than other individuals have. This is one great advantage of the Lamarckian theory, that a character which is constant in a whole species may be explained without assuming that it is either of selective value, or correlated with something of selective value. In fact, the idea of advantage or benefit to the organism belongs essentially to the selectionist view. All that the Lamarckian requires is that certain effects which he sees to be produced in the individual by response of living units to stimulation shall be cumulative, and then the modifications which we know to have occurred must necessarily follow. The effects of stimulation in its widest sense may be life-preserving or life-destroying; the hypothesis is that they are definite and cumulative.

Where the question of chance or fortuitous variations comes in I fail to understand. It is notorious that the selection theory assumes the occurrence of fortuitous variations, although with the understanding that fortuitous means unexplained only. But the assertion that the effects of conditions on the individual, or acquired characters, which alone I have considered in this article, are fortuitous is simply a contradiction of universal experience.

We find, therefore, on careful analysis, that Brooks conceives the effects of environment on the individual to be of two kinds; first, the kind of which the enlargement of a muscle by exercise is an instance, and, secondly, the others. Those of the first kind are due to structural adjustments for bringing them about. According to the Lamarckian view, all adaptations, at any rate all adjustments concerning whose action and efficiency there is no dispute, have arisen in the same way as the enlargement of a muscle by exercise; and the assertion that structural adjustments for rendering them possible exist in organisms is just what Lamarckians contend. Therefore, on this point Brooks agrees with Lamarckians; but whereas he supposes that these structural adjustments have to be explained, Lamarckians believe that they are merely the fundamental properties of protoplasm, namely, irritability and the power of assimilation and reproduction. The 'other' effects of conditions are those which confer no particular benefit on the organism. To take a trivial instance, a young man who takes to an open-air colonial life develops muscles and skeleton and senses to such an extent that he can ride, shoot, and work with marvellous skill and endurance. At the same time his

skin is tanned brown by the sun. The former changes are adaptive and beneficial; the latter would not usually be called an adaptation, though it may on investigation be found to be beneficial. It is an instance of the 'other effects' of the environment, and is obviously not haphazard, but definite and constant. Brooks asks if there is any evidence that the effects of the environment are inherently beneficial, and the reply is shown by the above considerations to be that, whether beneficial or not, the same conditions acting constantly produce a definite effect. We are here considering, not congenital variations—those which, like a supernumerary digit, arise in a single individual without obvious relation to the conditions of life—but exclusively the effect of external conditions on the individual during life.

Brooks thinks that natural selection, if it act at all, must result in adaptation, and that therefore it is a much more plausible theory than the Lamarckian. But this is begging the question: natural selection means the survival of adaptive variations, and the question is the origin and cause of such variations. Lamarckians do not admit that such variations have been proved to occur without the direct action of the conditions of life; therefore to assert that natural selection must produce adaptation is to assume the very proposition in dispute. Even if it were proved that the variations were independent of the conditions of life, the mere selection of them in itself would not be an explanation; we should still want to know the true causes of them.

J. T. CUNNINGHAM.

SOME NEW BOOKS.

Agassiz.

Life, Letters, and Works of Louis Agassiz. By Jules Marcou. 8vo. Two vols. Pp. xxii., 302, and x., 318, with vi. pls. New York and London: Macmillan & Co., Ltd., 1896. Price 17s. nett.

For the writing of this biography Mr. Jules Marcou has many qualifications. As a compatriot of Agassiz in both Old and New Worlds, as an intimate acquaintance and occasional collaborator, he stands throughout on familiar ground. The French writings of Agassiz of course present no difficulty to him, while his mastery of English is remarkably exemplified in the fluent pages of the work itself—one or two mis-spellings, and the use of "editor" in one instance where "publisher" seems to be intended, are the only gallicisms that have caught our eyes. But it is still more important that, to a knowledge of the facts, Mr. Marcou adds a singularly outspoken nature. This, and the lapse of time, enables him to refer to passages in the life of Agassiz that were slurred over by previous biographers. We will not go so far as to say that there are none living who will be offended by Mr. Marcou's remarks, but his attacks are fairly impartial and add piquancy to a book that is far from dull. The relations of Agassiz with Desor, Schimper, H. J. Clark, J. D. Forbes, and others are expounded in a manner that seems very free from personal bias, though there is perhaps a little prejudice against the Englishmen who did not at once appoint Agassiz professor at Oxford or head of a department in the British Museum, and this creeps out in the words, "English geologists are always strongly disinclined to accept any new truth if discovered by foreigners." But on the whole the book bears the stamp of truth, praise and blame of its subject being equally distributed; it affords a fair estimate of the scientific position and capacities of Agassiz, and is probably as near an approach to a final appreciation and biography of the great naturalist as we can expect to receive. To this it may be added that the book is well arranged and adequately illustrated, that it contains a list of all writings connected with the life of Agassiz, an account of all portraits and memorials of him, a bibliography of 425 writings by him, and a careful index. All working geologists and zoologists, for whom and not for the general public the work is intended, may be urged with confidence to its perusal if not to its purchase.

It is not contended that all scientific men of to-day will endorse the judgments of Mr. Marcou, for his scientific opinions are not altogether those now in favour. These, however, place him in more sympathy with his subject, who was himself a protestant against the evolutionary movement, and are easily discounted according to the private tastes of each reader. People in England, for instance, are not likely to concede that "from 1795 to 1873 these two savants [Cuvier and Agassiz] 'de très grand envergure' gave to natural

history the most important impulse which it has ever received" (II., p. 233). But perhaps we do not realise the enormous benefit that American science derived from the enthusiasm of Agassiz. He who had created the scientific centre of Neufchatel, embraced and invigorated a continent as easily as a city. One of his many improvements, the manner of publishing scientific writings, is well brought out by Mr. Marcou. Before his arrival most important works on American zoology had been published in Europe, and the scientific reports occasionally issued by the Government were in bad type on worse paper. Agassiz, with his European draughtsmen and lithographers, transferred to Cambridge the example begun in Switzerland, and American scientific publications soon became, as they have remained, the envy of the world. Would that an Agassiz might pay a visit to H.M. Stationery Office!

But, however great the impulse that Agassiz gave to the local investigation of nature, there can be little doubt that, as regards science in general, if we set aside the Glacial theory, the estimate of Mr. Marcou is too high. Those who have studied the works of Agassiz know well enough what a mine they are of accurate facts and brilliant suggestions. But his "Essay on Classification," the book that summed up his philosophy of zoology, a book well worth the careful study of all naturalists, has been simply shelved by the majority in favour of the "Origin of Species." This is a fact that it would be idle to deny, quite apart from any discussion as to the relative value of the two works. The world of zoologists was panting for a revelation, for some guiding principle, and it was not from Agassiz that the revelation came. This, indeed, is fully realised by Marcou, who forcibly contrasts the "classifiers and pioneer naturalists," including Cuvier, Agassiz, Owen, d'Orbigny, Deshayes, Ed. Forbes, Thomas Davidson, Pictet de la Rive, Herman von Meyer, Barrande, Lartet, and Cotteau, with the "philosophical naturalists," most of whom appear, according to our author, to lack some of their senses, being blind, or deaf, or invalids, or hermits. But even Agassiz said, "facts are stupid things until brought into connection with some general law," and after all it is the philosophers who give the most enduring and the widest impetus; and so Mr. Marcou—who, like Balaam, cannot help speaking the truth—admits of Cuvier and Agassiz that, "both saw facts and observed them sharply, but neither thought to link them by theories calculated to lead to the discovery of other facts. They were 'terre-à-terre' naturalists, while Lamarck, Geoffroy Saint-Hilaire, Darwin, Huxley, looked forward to the future, prophesying, and always ready to call to their help suppositions and probabilities."

Mr. Marcou of course does not mean that Agassiz never theorised. On the contrary, he tells us elsewhere that he speculated too much. But the truth seems to be that his speculations, like so many of his enterprises, were rashly entered upon and thrown aside by their author for want of patience to undertake the continuous drudgery necessary for their development.

This, then, is how Agassiz appears to us after the perusal of this book. An insatiate collector, a careful and penetrating observer, a lucid describer, a rapid worker,

> "A mortal, built upon the antique plan,
> Brimful of lusty blood as ever ran,
> And taking life as simply as a tree."

His unbounded energy initiated vast schemes, but lacked the perseverance and organising power to bring them to a successful issue.

He lived for the day on the promises of the morrow, letting the future take heed for itself. The Museum of Comparative Zoology at Harvard, now ably directed by his son, was the one practical object he set his heart upon, and remains an enduring monument to his vigour and to the "persuasion" that "fondled in his look and tone." His own enthusiasm and reverence for natural objects inspired others "to feel the Seeker's noble zest," but did not always direct and restrain them. Confident in himself, yet longing for the applause of his fellows, he owned a charm that overcame men of all classes:

> "His magic was not far to seek,—
> He was so human! . . . Still himself he bare
> At manhood's simple level, and where'er
> He met a stranger, there he left a friend."

Let alone princes and politicians, it is no small thing to say of a man of science that he was the subject of three of the noblest utterances of the poets of his adopted country, Longfellow, Whittier, and Lowell.

> "In all voices known to her
> Nature owns her worshipper."

F. A. B.

METHODS IN HISTOLOGY.

LEITFADEN FÜR HISTOLOGISCHE UNTERSUCHUNGEN. By Bernard Rawitz. 2nd Edition. Pp. xiv., 148. Jena: Gustav Fischer, 1895. Price 3 marks.

THE second and improved edition of Dr. Rawitz's treatise on histological methods will be welcomed by all histologists, whether students or researchers. The work is avowedly intended to be a guide to students of vertebrate histology, that is to say, of histology as the word is understood and restricted by anatomists, physiologists, and others who are engaged in studies directly or indirectly connected with medicine. It will, therefore, be of more value to the student or researcher in a medical school than to the general zoologist, who will search in vain for any of those methods, now so numerous and detailed, for killing and preserving animals, marine and otherwise, in as natural and lifelike a condition as possible. Such methods form nowadays a most important part of zoological technique, especially for collectors or marine zoologists.

Although Dr. Rawitz devotes chapter i. of part i., and a portion of chapter i. in part ii., of his work to methods of studying living material, it is the constituent cells and tissues, while fresh and still living, of higher animals that are included by him under this term, and not the living animal as a whole. In short, this work, as its title implies, is purely and simply histological, and as we have said, histological in the limited sense of the word. Since, however, the greater part, if not all, of the methods given in this book admit of application, with such modification as circumstances may render necessary, to invertebrates as well as vertebrates, the book may be safely recommended to those histologists and embryologists whose studies are not restricted to the latter portion of the animal kingdom.

The intention of the author has been to make his book, on the one hand, a useful guide to the beginner who, having been through a course of microscopical study, wishes to commence original investigations on his own account, and, on the other hand, a work of reference in which the experienced investigator may find brought together the many important methods of research recommended in scattered memoirs on various subjects. The book is divided into two parts, the first being a classified description of the methods themselves, the

latter explaining their application to special instances. Part i. is divided into eleven chapters, with the following headings; Investigation of Living Material, Methods of Cell Isolation, Fixation and Hardening, Decalcification and Bleaching, Imbedding, Section-cutting, Staining, Metallic Impregnation, Putting up of Preparations, Injection Methods, and, finally, Drawing and Plastic Reconstruction. The first, fourth, eighth, and last of the above chapters have been added in the present edition. In part ii. the application of these methods is explained in a series of chapters divided according to the tissues and systems of the vertebrate body, such as The Cell, Muscular Tissue, Nervous System, and so forth.

The author does not aim at giving a complete account of all methods that have been recommended, but at selecting those which seem to him really useful. Several processes which were described in the first edition have been eliminated in the present one. There is certainly, however, no histological method of real value which is not to be found in the book. Each chapter in part i. is preceded by a short introduction, which contains nothing new for the experienced investigator, but is intended especially for the beginner, who will find there, not only many hints and "tips" most helpful in practice, but also valuable explanations as to the why and the wherefore of the processes employed. The application of the methods to particular instances, which is described in the second part of the book, is also a great aid to the student. It may be doubted, for instance, if an inexperienced person could obtain good preparations by the famous method of Golgi, or some of its numerous modifications, after having read only the short account given in chapter viii. of part i., were it not for the further description given under the heading Central Nervous System in part ii. In part ii. will be found also a number of special methods, applicable only to particular instances, such as Blochmann's method for removing the jelly from frogs' eggs, not mentioned in part i.

For imbedding in paraffin the author recommends chloroform as by far the best medium for delicate objects, not only on account of the slowness with which it diffuses, but also because it is possible—and indeed necessary—to drive out every trace of chloroform by evaporation, so that the objects are imbedded in perfectly pure paraffin, the melting-point of which has not been lowered during the process. It may further be pointed out that objects so imbedded can be preserved for any length of time far better, and with much greater safety in packing and travelling, than in alcohol or any other preserving medium. This method is especially useful when large quantities of delicate material are collected during a short time, and cannot be worked out at once, as during visits to marine laboratories.

In the chapter on section-cutting we miss a description of any method for the orientation of minute objects, such as the well known one of sticking them with glycerine and albumen to thin slices of liver, as already described in a former number of this Journal (NAT. SCI., vol. iii., no. 18, p. 122; Aug., 1893). Among various methods of sticking sections to the slide, the glycerine and albumen mixture is especially recommended, while the advantages of the distilled water method are also pointed out. The latter method is certainly dangerous, however, when the sections have to be soaked afterwards for a considerable length of time in staining fluids, especially if the sections are not very thin, but it may be made perfectly safe by adding a very small quantity of glycerine and albumen to the distilled water, *i.e.*, one or

two drops of the former to about 10 c.c. of the latter. This method is especially recommended for series of sections—ribbons—which are slightly curved or crumpled, as the water causes them to flatten out, while the amount of albumen present, though sufficient to make the sections stick on, is too infinitesimal to be appreciably coloured by the staining fluids.

The chapter on staining is certainly one of the most valuable in the book, and the portion dealing with the stains is divided into three parts, headed respectively "Substantive," Adjective," and "Vital Staining Methods." By "substantive staining methods" the author denotes all processes where the staining fluid, as made up, acts directly upon the substance to be stained. By "adjective staining methods" are meant those processes in which the substance is first treated with a mordant, used in a solution separate from the stain, and not combined with it as in the ordinary combinations of alum and other salts with carmine or hæmatoxylin, commonly employed in laboratories. Among adjective staining methods are given the famous hæmatoxylin methods of Heidenhain and Weigert, as well as a remarkable new method of the author's, in which the mordant used is a solution of tannin and tartar emetic, followed by fuchsin, safranin, or other anilines as a stain. This is the first time an adjective method has been employed for an aniline stain, and it is said to give results both curious and valuable. The ordinary staining reaction of the aniline is, as it were, inverted; the fibres of the karyokinetic spindle, for instance, are coloured, and not the chromosomes. The centrosome is also said to be very distinctly shown in the resting cell.

These adjective staining methods, first invented by Heidenhain, and since elaborated by him and by others, are among the most valuable of modern microscopical methods, and Heidenhain's later method with iron, alum, and hæmatoxylin is rapidly establishing itself in zoological and embryological, as well as in histological, technique. It may be especially recommended for showing micro-organisms in cells or tissues, not only in pathological material, but also in cases where such organisms occur normally, as in *Pelomyxa*, for instance, where this method shows the curious bacteria in sections with a clearness not attained by any other stain.

The various methods of staining *intra vitam* with methylene blue form the third section of the chapter on stains. These important processes are perhaps not treated with the fulness they deserve, and the book has appeared just too soon for it to contain an account of Bethe's method for fixing the methylene blue in the tissues, which is of so great importance that it may be worth while to give a brief account of it here, abstracted from Bethe's memoir on the nervous system of *Carcinus mænas* in the *Archiv. für Mikroskopische Anatomie*, vol. xliv., p. 585; 1895.

The stain is applied either by injecting the animals with it, or by placing pieces of tissue in a weak solution. For injecting Crustacea a 1½ per cent. solution in normal saline solution was employed. For fixing the stain in the tissues, the following solutions are used:—(1) For vertebrates: Ammonium molybdate, 1 gr.; distilled water, 10 cc.; H_2O_2, 1 cc.; H Cl, 1 drop. (2) For invertebrates: Ammonium molybdate, 1 gr.; distilled water, 10 cc.; H_2O_2, ½ cc.

The fixative should be used freshly made up, and during the process of fixation should be surrounded by a freezing mixture. Small objects are fixed for two to three hours, larger ones, up to 1 cc. in bulk, for four or five hours. After fixation the objects should be thoroughly washed in distilled water and dehydrated with alcohol,

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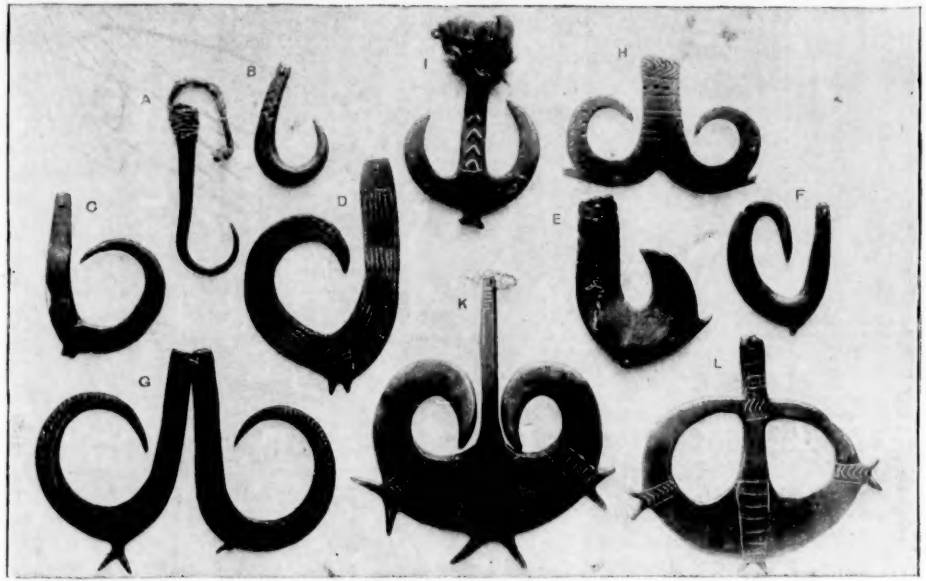

EVOLUTION OF *Sabagorar*.

A. Ordinary Fish-hook of Turtle-shell. B–L. Series of Ornaments of Turtle-shell probably derived from Fish-hooks.

(Being Fig. 44 of Professor A. C. Haddon's "Evolution in Art," the block kindly lent by the publishers, Walter Scott, Ltd.)

[To face p. 337.

the process of dehydration being made as rapid as possible. Clear with oil of cloves, or, better, with xylol, taking great care to work out all trace of alcohol. Mount in Canada Balsam, or imbed in paraffin or celloidin by usual methods. Objects so fixed may be further treated with hardening or staining reagents, but strong mineral acids and alkalies are to be avoided, hence ammonia carmine or borax carmine cannot be used. They may be stained in bulk or sections with alum carmine or cochineal, or with any anilines. Hæmatoxylin does not give good results on account of its blue colour. The objects when fixed can be treated, without further preparation, with chromic or picric acid or potassium bichromate. If it is required to treat with $HgNO_2$ to show epithelia, the HCl in the fixative must be replaced by HNO_3, and after fixation the washing must be very thorough, to avoid formation of silver molybdate. Osmic acid is best applied by adding it to the fixative fluid after the objects have been in it for some time.

Rawitz's chapter on staining ends with a useful appendix giving the synonymy of the various aniline stains employed in histological technique. One and the same stain is often known by as many as four names, or even more, and this frequently causes very great confusion to beginners. We have heard of a student who, being directed to stain a tissue with vesuvin, complained that he could not find this stain in the laboratory, although bismarck brown was staring him in the face on his own shelf. How many people, not specialists in this subject, know that magenta, fuchsin, rubin, and aniline red are but four different names for the same thing?

The chapter on drawing and plastic reconstruction will appeal especially to the morphological student, who often has to reconstruct the structure of a minute object from a series of sections. The usefulness of the book is much increased by the complete index, and it is in every way a work to be recommended, especially to the class of students for whom it is primarily intended. E. A. MINCHIN.

THE NATURAL HISTORY OF ORNAMENT.

EVOLUTION IN ART: as illustrated by the Life-histories of Designs. By Professor A. C. Haddon. Pp. xviii., 364. London: Walter Scott, Ltd., 1895. Price 6s.

THE great interest which in recent years has centred round the study of art from a scientific standpoint, and the investigation of the natural history, so to speak, of designs, amply justifies the numerous books and essays treating of this aspect of the subject, which have lately come into existence. This field of research is indeed a wide and a fascinating one; and as yet, for the most part, the surface alone has been skimmed, the depths still remain to be probed, but the activity of researchers bids fair to secure a rapid development of the subject.

The present volume is the latest addition in this country to the study of the "Life-histories of Designs," and will be welcomed as a concise and suggestive essay, tending to advance our insight into the nature and *raison d'être* of art designs. Professor Haddon lays stress upon the now generally admitted fact that the ordinary biological deductions and lines of inquiry are applicable to the study of the growth, affinities, and migrations of designs, and he brings the eye of a trained zoologist to bear upon the subject.

As regards the material with which he deals in this book, there is but little which can be called new. He has borrowed very freely, and nearly all the instances cited and most of the theories advanced have been published elsewhere, as is also the case with practically all the illustrations. The first section of the book, dealing with the

"Decorative Art of New Guinea: as an Example of the Method of Study," is an abridgment of Professor Haddon's own interesting paper published in the "Cunningham Memoirs" by the Royal Irish Academy, and gives the main points indicated in that monograph. The remainder of the book, in which the classification is a prominent feature, is mainly built up of quotations from various well-known writers on the subject, such as Colley March, Stolpe, Holmes, Cushing, Goodyear, and others, who have with great skill treated of the various branches of the subject of Evolution in Art. The author adds his own views where necessary, and at the end gives suggestions as regards the scientific method of studying Art. His classification of the various modes of origin of designs, and the ways in which they become modified, will be found to be a useful one, as will also his grouping of the various reasons for which objects are decorated.

Some of the instances of "life-histories" of designs are very complete and convincing; as, for instance, the phylogeny of the *sabagorar*, or hook-like ornament of turtle-shell of the Torres Straits (Plate VI.). "Fish-hooks (A) are used in pairs, being fastened at each end of a piece of fine string, which, in its turn, is tied at its middle to the fishing-line proper. When the piece of twine with its hooks was thrown round a girl's neck, the two hooks would often hang down her back shank to shank. Two *sabagorar* similarly arranged occur in the British Museum collections. What more natural than that this should be noticed, and to save the trouble of making two *sabagorar*, a double one should be cut out of one piece of turtle-shell. The more remotely from the fish-hook did the *sabagorar* vary, the larger it became, and in some instances the double form became of considerable size, and the hook portion acquired a slight spiral curvature (K). In one modified specimen the hooks are actually fused with the shank (I). It will be also seen that divergent Λ-like processes often occur on the *sabagorar*, but are never found on the fish-hook" (pp. 76–78).

In some cases resemblances are apt to mislead, and we think that Professor Haddon is in error in suggesting (p. 231) that the Burmese silver "shells" used as currency represent actual shells once current as a medium of exchange. Although it is true that actual shells have been, and are still so used in Burma, surely the silver "shell"-money, so-called, is merely an accident of manufacture, a condition brought about in the crucible, and attainable by the metal only when of a certain degree of purity. The peculiar form, undesigned in the first instance at any rate, is valued as a guarantee of assay, and probably has no connection with the imitation of natural shells.

In dealing with what he calls "heteromorphs," that is, cases in which a design is composed of an amalgamation of two or more designs, Professor Haddon touches upon a cause of variation which is frequently made too little of by writers and students of the histories of art forms. It may safely be predicted that the greater number of decorative designs will prove to be heteromorphs of a more or less definite nature. The influence of one or more designs over another, which tends to produce a new design partaking of the nature of each parent, is very common, and renders the tracing of phylogenies a complex and difficult pursuit. One is indisposed to regard heteromorphism as "a kind of disease which may attack" designs; or to class it under the heading "The Pathology of Decorative Art." It is merely hybridisation, and, like that process in the animal kingdom, is capable of producing noble and improved forms, while in contra-

distinction to animals and plants, patterns of even widely different "species" may hybridise freely, and with excellent results.

The book is well indexed, and the illustrations are numerous and well chosen. The eight plates at the end of the volume are taken *en masse* from a paper by Dr. Colley March. It is a pity that the publication seems to have been hurried. In the copy before us, at any rate, the illustrations, which cannot have been dry when the sheets were bound up, have left indelible impressions upon the opposite pages which are eminently unsightly: an easily avoidable and therefore inexcusable blemish. This, however, does not impair the scientific value of the work, which should appeal to a wide class of readers.

HENRY BALFOUR.

PERCIFORM FISH.

CATALOGUE OF THE FISHES IN THE BRITISH MUSEUM. By G. A. Boulenger. Second edition. Vol. i. Pp. xix., 394, with 15 plates. Published by order of the Trustees. London, 1895. Price 15s.

IN 1870, Dr. Albert Günther completed his great enumeration of all known fishes, bearing the general title "Catalogue of the Fishes in the British Museum." Just a quarter of a century later the publication of a new enumeration was commenced as a second edition with the same title as Dr. Günther's work. The first volume now published has also the specific title "Catalogue of the Perciform Fishes of the British Museum. Second Edition. Volume First." The author of this new "Catalogue" is Mr. George Albert Boulenger, the work is cast on different lines from the first edition, and the secondary title is entirely different from any that has appeared before, and, therefore, it is only the supposed exigency of Museum administration that justifies the designation of the new work as an edition of the old Catalogue.

During the thirty-five years that have elapsed since the publication of the first volume of Dr. Günther's Catalogue, the nominal additions to the species have been very numerous, but still more noteworthy has been the deviation of many ichthyologists from the system therein proposed. Nevertheless the old system has continued to find favour with most European ichthyologists; consequently there has arisen a division into two "schools" one comprising most all the American ichthyologists, the second most of the others. Naturally curiosity will be excited to learn how both questions—numbers and system—are viewed in the new Catalogue.

Probably by most experienced ichthyologists it has been believed that many of the supposed new species would not stand the test of critical examination; but few, if any, will be prepared for the sweeping reductions which have found expression in the Catalogue of the Perciform Fishes. In the corresponding portion of the old work, mostly published in 1859, 393 nominal species of the groups under consideration were enumerated, but "only 317 were considered as apparently well characterised." In the present volume, only 375 species are recognised. This reduction is the result, not only of identification of many of the "doubtful species" of the old work, but of some considered "well characterised" and of most of the subsequently described species. Some of the identifications rather startle one; such is the degradation of the "*Batrachus gigas*, Günther," in the synonymy of the common *Epinephelus lanceolatus*. The supposed *Batrachus* was based on a dried head and has long been a puzzle; in view of the positive statement by so experienced an ichthyologist as Dr. Günther that "its appearance and structure" are those of a *Batrachus*, and the trenchant characters of that genus, no one has ventured to doubt that

the identification was correct for the family at least, although not for the genus, and it could only have been possible for one having access to the type to identify it as Mr. Boulenger has done. This is simply an example of the results of the facilities enjoyed by the author and the good use he has made of them. Undoubtedly, however, there will be dissentients from him in some of his reunions. No one, for instance, who has had the opportunity of examining carefully large numbers of the *Pomoxis annularis* and *P. sparoides* will concur with Mr. Boulenger in combining them in one species (*Pomoxys sparoides*, p. 7); Dr. Jordan did indeed hastily anticipate him in such a conjunction, but he very soon repented.

Notwithstanding the reduction of species, genera not represented by any species in the corresponding portions of the Catalogue of Acanthopterygian Fishes are numerous. The additions are of Centrarchidæ, *Elassoma* (p. 34); of Percidæ, *Ulocentra* (96), *Ammocrypta* (101), *Crystellaria* (103); of Serranidæ, *Percalates* (132), *Siniperca* (136), *Parascorpis* (145), *Dinoperca* (153), *Liopropoma* (154), *Odontanthias* (319), *Plectranthias* (331), *Dactylanthias* (333). These, of course, are independent of new genera based on previously known species.

The system Mr. Boulenger adopts is excellent, so far as his appreciation of the groups is concerned. The fact that the first volume of the new Catalogue is devoted to specialised acanthopterygians is due to the circumstance that he "was desired to begin the work with the Perciformes."

The "division Perciformes" is much more restricted than is Günther's homonymous division, the Squamipennes, Teuthididæ, and Scorpænidæ being the principal types eliminated. The families, also, are very differently limited, and the author has expressed the facts of osteology in his diagnoses of the families, and not simply given perfunctory descriptions of skeletons as addenda of curious but useless information. He has, too, for the first time, utilised the development or want of an ophthalmophorous shelf to the second suborbital as a family character, and has also availed himself of the mode of insertion of the ribs and the development of transverse processes for diagnostic purposes. The results are most satisfactory.

The Percidæ are identical with the family as understood by American naturalists, the Centrarchidæ include the typical Centrarchidæ, Elassomidæ, and *Kuhlia*, and the Serranidæ form a very large family with numerous subfamilies.

The relationship of the peculiar Pacific genus *Kuhlia* to the Centrarchidæ was entirely unsuspected, and Mr. Boulenger is the first to recognise the distinctness of its characters from those of forms with which it was formerly associated. Probably some will be inclined to consider it the representative of a distinct family—Kuhliidæ. Indeed, doubt must for the present be entertained whether the similarity of the vertebral characters of *Kuhlia* and the Centrarchidæ is not the result of independent acquisition from different sources rather than indicative of close genetic relationship.

The Serranidæ are those members of the old Percidæ which have the "second suborbital with an internal lamina supporting the globe of the eye," and include as subfamilies the Serraninæ, Grammistinæ, Priacanthinæ, and Centropominæ, besides others to be hereafter described. The last two subfamilies named are equivalent to the families Priacanthidæ and Centropomidæ of some authors, and are very natural groups, whatever may be their value. (We prefer to consider them of family rank.) The Serraninæ are extended by the addition of the Plesiopinæ, which previously had

been widely separated. The alliance of this group with the Serraninæ is at first startling, but there is certainly a superficial resemblance to *Callanthias*, a universally-recognised genus of the Serranids, and we are even prepared to discount the value of the interruption of the lateral line by the characteristics of *Callanthias*. We may, therefore, thank Mr. Boulenger for the approximately true position of the group, for the ascertainment of which he had the skeleton of one species—*Paraplesiops meleagris*. This being conceded, the differences between Mr. Boulenger and American naturalists would be reduced to differences of valuation simply, namely:—

Boulenger.	*Americans.*
Centrarchidæ	Centrarchidæ. Elassomidæ. Kuhliidæ.
Percidæ	Percidæ.
Serranidæ Serraninæ ... ... Grammistinæ	Serranidæ.
Priacanthinæ	Priacanthidæ.
Centropominæ	Centropomidæ.

This is quite a satisfactory agreement and a great improvement over the previous condition.

The genera admitted by Mr. Boulenger are as great improvements over those generally adopted as are the families. No other ichthyologist, except Bleeker, has been equally fortunate in appreciating the characteristics and relations of the numerous Indo-Pacific and Anthias-like forms. Many, however, will be disposed to delimit and sub-divide some large genera, such as *Epinephelus* and *Serranus*, but even here little more will be required than to elevate his sub-genera to higher rank, although a few of those may be sub-divided. Perhaps there will always be such differences of opinion, and when so much may be said on both sides, dogmatism is out of place; it is even possible that Mr. Boulenger may be more nearly in the right than those who have been more radical. It is not likely, however, that his views will prevail immediately everywhere.

The nomenclature of Mr. Boulenger's work is as good as the system. He is generally obedient to rules, and the differences between him and others likewise amenable to laws depend chiefly on the nature of those laws. Mr. Boulenger adopts the twelfth edition of the Systema Naturæ as the starting point of binomial nomenclature, demands a diagnosis (and not merely a typonym) for every new genus, and requires that the name shall accord with Latin usage and be appropriate.

For one reason or another, seven of the genera of the new volume will be subjects of differences of nomenclature. These are:—

Page	Boulenger.	Diverse.
50	*Lucioperca*, Flem., 1822.	*Stizostedion*, Raf., 1820.
104	*Aspro*, C. V., 1828.	*Zingel*, Cloquet, 1817.
109	*Acerina*, C., 1817.	*Gymnocephalus*, Bl., 1795.
133	*Ctenolates*, Günth., 1871.	*Plectroplites*, Gill, 1872.
144	*Pomodon*, Boul., 1895.	*Hemilutjanus*, Blkr., 1876.
271	*Cromileptes*, Blkr., 1874.	*Serranichthys*, Blkr., 1859.
306	*Gilbertia*, J. & E., 1890.	*Hypoplectrodes*, Gill, 1862.

Which ones of these alternative names shall be adopted time alone can tell. We sympathise with Mr. Boulenger as to several cases, but feel compelled to adopt the other names, although dis-

tasteful to us. Probably the difference above noted may even be reduced to not more than five, as the data available to Mr. Boulenger seem to have been insufficient in two cases. *Plectroplites* and *Hypoplectrodes* were rejected because they were supposed to have been justified by "no definition" before the synonyms. It is true they were not defined at the pages noted by Mr. Boulenger, but a little later the differential characters were mentioned, characteristics of *Plectroplites* having been given in 1863 (*Proc. Acad. Nat. Sci. Philadelphia*, p. 83), and some of *Hypoplectrodes* in 1871 (Poey, *Ann. Lyc. Nat. Hist. N.Y.*, x., p. 45). It may be added that *Cromileptes* is rather a synonym of *Epinephelus* than a name available for a distinct genus, and was only used by Bleeker on account of his peculiar view as to type-species.

In fine, we heartily congratulate all ichthyologists and naturalists generally on the publication of a work which so well embodies the scientific spirit of the age in its system, the grasp of the subject as manifest in the treatment of the specimens, literature, and synonymy, and obedience to law as observed in the nomenclature. Naturally, we must look forward with impatience for succeeding volumes.

THEO. GILL.

THE DOG'S SKULL AND OTHER MATTERS.

THE SKULL OF THE DOG. By Walter E. Collinge, F.Z.S. Pp. viii., 124, with 11 woodcuts. London: Dulau & Co., 1896. Price 5s.

THIS little book, with its bright red cover, bears a superficial resemblance to one of Macmillan's excellent "Manuals for Students," but the plagiarism, if such it be, is not carried farther; in fact, the thickness of the paper, the large type, and the width of margin are rather suggestive of book-making. One of the author's reasons, as stated in the preface, for selecting the skull of the dog for description, is that it is "easily procurable in both adult and young stages"; but the enterprising student who provides himself with young stages will derive but little assistance from the book, for there is no chapter on the development of the skull and hardly more than a mention of the milk dentition.

The book is divided into five chapters, the first of which is introductory, while the second relates to the histology of the connective tissues. The third chapter, describing the skull of the dog, occupies only 44 pages out of a total of 124. The matter is reliable in the main, but to describe the vomer with the bones of the olfactory capsules is calculated to confuse the student when he comes to study the skull of the lower Vertebrata, and to give him false ideas concerning the true nature of the mammalian hard palate. Since, also, by "upper jaw" in animals from the bony fishes upwards, is usually understood the tooth-bearing bones, premaxilla and maxilla, the statement that the palatoquadrate cartilage in the embryo dog forms the foundation of the upper jaw of the adult is liable to misconstruction. As regards the illustrations, which are indifferent in quality, the shape of the tympanic bulla in fig. 3 calls for special censure, and it is to be regretted that the dog who furnished his mandible as the model for fig. 4 was not endowed with the full complement of teeth.

In chapter iv., Windle's tables, occupying some sixteen pages, of cranial and dental measurements of the different breeds of dogs might surely have been omitted, especially as the student, in referring to them, has to guess whether the numbers represent millimetres, inches, or yards, and may, moreover, be pardoned if he fails to evolve the "index," when given the total length of a skull and its zygomatic

width. To form the subject of the last chapter, a glossary of osteological terms has been thrown in, apparently as an afterthought, for it is not specially related to the subject of the book. The derivation, or rather the author's idea of the derivation, is given for most of the words, and here the classical scholar may gather much information of which he was erstwhile ignorant. He may learn, for instance, that *zugnum* means "I yoke together"; *trochas*, "I roll or run round"; and *phusis*, "I grow"; while, if his predilections lie in the direction of Latin, he may learn the meanings of a great many words which do not occur in the dictionary. The author has evidently been unable to decide whether to render the Greek letter "upsilon" by *u* or *y*, and so gives us an equal quantity of both. This, of course, has the advantage of relieving the monotony of an otherwise unattractive glossary, and further variety is afforded by deriving "pterygoid" from *pteron*, and "basipterygoid" from *pterygion*, although the roots of "entopterygoid" and "mesopterygoid" are recorded as *pteryx* and *pterux* respectively. It is also not encouraging to the inquiring student to be told that the supratemporal is "a bone of the skull in certain Vertebrata," more especially as splenial, prefrontal, and ectopterygoid are described in exactly the same terms.

In spite of these deficiencies which the carping critic sets himself so assiduously to ferret out, there is abundant evidence of the patience and perseverance which the author has exercised in his endeavour to produce a concise and reliable students' manual. We cannot say that this endeavour has been crowned with unqualified success, and it is to be feared that the author will not greatly benefit by the publication of the work. There was no demand for the book, and the thirty closely-printed and well illustrated pages devoted to the skull of the dog in Flower's "Osteology of the Mammalia," to which students have had recourse in the past, still remain far and away the best source of information on the subject.

W. R.

Plankton Gastropods.

Die Gastropoden der Plankton-Expedition. Ergebnisse der in dem Atlantischen Ocean von Mitte Juli bis Anfang November, 1889, ausgeführten Plankton-Expedition der Humboldtstiftung. Von Dr. Heinrich Simroth. Pp. 206, twenty-two plates, seventeen cuts. Kiel und Leipzig, 1895.

The Gastropoda collected during Professor Hensen's Expedition in the "National" were confided for examination to Dr. Heinrich Simroth, who, as might have been expected, found his investigations beset with no ordinary difficulties, owing partly to the small size of the organisms and the distorting effect of reagents upon them. Comparatively few of them could be named, as in the present state of our knowledge it is impossible to refer them with certainty to particular adults.

The first section of this report is devoted to the genus *Ianthina*, and numerous features in its structure and life-history are discussed. The radula is divided quite to the base into two halves, and the sides of the mouth are supported by lamelliform jaws. The teeth appear to serve as a sieve for straining the minute plankton on which the animal feeds. The float consists of different elements; the propodium supplies a homogeneous mucus, of which the air-bubbles are formed; the mesopodium a thread-like byssus; there is, further, a colour gland to which the varying tint of the float is due. The eggs (to the number of 4,000 in *I. umbilicata*, 400,000 in the larger species) are deposited on the under surface of the float by a protrusible ovipositor, and the process takes place all at once. Where a float shows traces of two

separate operations, it is because a portion of a last year's float has remained.

Of the larvæ, those of Lamellariidæ were characterised by the possession of a broad, hyaline "scaphoconch." One of the commonest was that known as *Echinospira*, which is probably the young of *Lamellaria perspicua*. The remaining larvæ are classed in seventeen categories, among which only a few can be mentioned here. *Macgillivrayia*, the larva of *Dolium*, is widely distributed; it has long bristles, and the operculum has a vertical median ridge on the inner surface for the attachment of the columellar-muscle, besides which a system of comma-like dots oblique to the ridge indicates incipient torsion. In *Sinusigera*, a collective name for the larvæ of *Murex*, *Purpura*, *Bela*, *Triforis*, *Cypræa*, etc., the notch is correlated with the protrusion of a sail-like lobe of the velum. In this group the metamorphosis of the shell is very distinct: the larval shell (prosopoconch) is often clearly distinguishable at the apex of the definitive shell (teleoconch), and usually bears at its own apex the original embryonic shell (embryonoconch). One specimen suggested the incipient shell of a *Spirula*, but, being smaller than would have been expected, was probably referable to a species of *Cæcum*.

Notwithstanding the difficulties of the enquiry, and the uncertainty of the specific determinations, a few general principles may be enunciated. Slender shells, the length-breadth ratio of which exceeds 5 : 2 occur only on the coasts, not in the plankton. All the larvæ are provided with enormous velar processes, with a complicated double series of cilia, and a strong longitudinal retractor muscle. They have a lacunar structure correlated with respiration. The sole of the foot, though distensible, is extremely small, and the proboscis is wanting, though its sheath occurs as a large pigmented sac. The radula could not be used for systematic purposes, as its lateral and marginal teeth showed different relations from those obtaining in the adult. With respect to the shell, the embryonoconch is of structureless conchiolin. The most primitive complication is the hair, resulting from some active secretion on the mantle-margin; longitudinal and transverse knobs and prominences arise also in consequence of irregular secretion, and in their simplest arrangement are compared with the staves and hoops of a barrel: sometimes their deposition takes place at an angle of 45° to the axis. The colour of the shells is either yellow-orange or blue-violet—that is to say, in the main, of one of two complementary tints. The blue series is the result of exposure to the tropical sun; only forms which dive are colourless, and only swimming forms are phosphorescent.

It will be obvious, even from this brief outline, that the Memoir contains many interesting and valuable facts and much suggestive comment. It is beautifully illustrated, and we congratulate Dr. Simroth on his courage in undertaking such a task and on the success which he has achieved in discharging it. W. E. H.

ECHINODERM EMBRYOLOGY.

TEXT-BOOK OF THE EMBRYOLOGY OF INVERTEBRATES. By E. Korschelt and K. Heider; translated by E. L. Mark and W. McM. Woodworth. Part I. 8vo. Pp. xvi., 484. London: Sonnenschein, 1895. Price 15s.

THE science of animal embryology advances so rapidly that English students have anxiously awaited this long-promised translation of the leading German manual. Delay has not, however, had the result of rendering the book out-of-date. On the contrary, it is a considerable improvement on the German edition, owing to the insertion of

numerous notes by the original authors as well as by the two competent translators. For the present volume, which deals with Porifera, Cnidaria, Ctenophora, Platyhelminthes, Orthonectidæ and Dicyemidæ, Nemertini, Nemathelminthes, Acanthocephali, Rotatoria, Annelida, Sipunculidæ, Chætognatha, Enteropneusta, and Echinoderma, 130 additional papers have been consulted. While there are no additions to the literature of Ctenophora, there are no less than thirty to that of Echinoderma, and this without reckoning the numerous papers on experimental embryology, most of which have dealt with the echinoderm ovum. No papers after 1894 are included, so that the latest important works by Bury and MacBride are not discussed. With regard to the general questions of the homologies and relations of the classes of Echinoderma, the authors hold views with which we agree in the main. For instance, "Even the conception of the homology of the plates founded by Lovén and championed by Carpenter, especially those which in the different groups of Echinoderms lie about the apical pole, is not to be considered as assured." They reject Semon's ancestral *Pentactæa*, thinking it "more justifiable to search for the ancestral forms of the Echinodermata among the existing material which is offered to us by palæontology." The search will, we fear, take a long time. It is recognised that the origin of the radial structure must have been due to fixation, hence the common ancestor is sought among stalked forms; this is not absolutely necessary, for the evolution of a true stalk was a slow process, and mere fixation would probably have effected all that the theory requires. The Cystidea are no doubt the most ancestral of the known classes, for we agree with the authors in regarding the Holothurioidea as degenerate in some respects; but there have yet been found no certain transitions between the Cystidea and the other classes, except perhaps some of the Crinoidea. As to the internal structure of the primitive Cystid—that elongate sac, devoid of radial symmetry, with irregular plates developing in its integument, and attached by what was the pre-oral lobe of its young—we know nothing. Here embryology should shed its light, but affords the merest glimmer. The forms of the larvæ have been to a large extent secondarily acquired, and the very one that should be of most help is the most modified, namely, the crinoid larva, that is to say in *Antedon*, where alone we know it. It seems probable that the ancestor was bilaterally symmetrical, and to some extent segmented. The affinities with the *Tornaria* of *Balanoglossus* will doubtless serve as our best clue.

We cannot leave this subject without observing that an account of the development of *Antedon* that takes no note of the anal plate, in either text or figures, is singularly deficient. There is also an inadequacy in the account of the infrabasalia, which do not always have the same arrangement, so that the comparison with the Ichthyocrinoidea falls to the ground. It might have been added that the homology of the first formed stone-canal and water-pore with the stone-canal and madreporite of other echinoderms is confirmed by palæontological evidence, in so far as a similar madreporite is sometimes developed in connection with it. In a future edition the slip at the bottom of p. 453, by which *Rhizocrinus* is printed instead of *Antedon*, should be corrected, as it alters the meaning of half the page.

The remainder of this work is to be translated by Dr. H. J. Campbell, of Guy's Hospital Medical School, who will doubtless be as successful as Drs. Mark and Woodworth, and who will, let us hope, steer clear of their "fundamental" error. F. A. B.

FRENCH BEETLES.

FAUNE DE FRANCE: COLÉOPTÈRES. Par A. Acloque, with a Preface by E. Perrier. Pp. 466, with 1,052 figures. Paris: Baillière, 1896. Price 8 francs.

THIS book is to form one of a series of four volumes that shall comprise the whole Fauna of France, with the object of enabling an enquirer to find the name of any animal by a series of dichotomic tables. The larger part of the book is occupied by these tables, but there is an introduction containing a general account of the structure of insects.

As works of this class are much in vogue on the Continent of Europe, we must suppose that they are useful; or perhaps rather that they are considered to be so by a section of the public; we believe, however, that they give rise to much disappointment. In order to find the name of a beetle, a student must commence—supposing him to have ascertained that he has a beetle before him—with the table of families, and trace the dichotomic divisions until he ultimately gets to the species; before arriving at this, however, he must encounter so many doubts and difficulties that he will probably abandon the task as hopeless; should he refuse to allow his doubts to deter him, and continue till he has run down to the species-name, we should say, from our own experience of other books of the kind, that he will as often as not be guided to a name that belongs to something else.

This first instalment of Mr. Acloque's "Faune" is a model of condensation. The author has lightened his difficulties by omitting "litigious" species; but the volume as it stands must include, we should imagine, somewhere about 5,000 species. Detailed criticism of either the introductory matter or the tables would be out of place here; but as other volumes are to appear, we may remark that an alphabetical index—extending as far as genera—is necessary in a work of this kind, though wanting in this volume. The book is well printed, the illustrations are sufficiently good to be useful, and Professor Perrier's preface is a gay fragment of autobiography.

D. S.

PALÆONTOLOGY.

GRUNDZÜGE DER PALÆONTOLOGIE (PALÆOZOOLOGIE). By Karl A. von Zittel. 8vo. Pp. viii., 971, with 2,048 text-figures. Half morocco. München und Leipzig: R. Oldenbourg, 1895. Price 28s.

PROFESSOR VON ZITTEL'S Handbook of Palæontology is so well-known, and, if it were not, we have so recently published a critical account of it (vol. iv., pp. 222–225), that a lengthy review of the present work, which is a condensation of the larger one, is quite unnecessary. It will certainly be welcomed by those students who could ill afford to pay for the indispensable but expensive "Zittel." In addition to being compressed, the work has in some respects been brought more up to date: it is true that this process might have been carried out with more rigour, and there are advanced palæontologists who will be horrified at the retention of what they regard as some exploded absurdities; but as most of the important recent literature is referred to, the student who desires to reach this advanced stage is put in the way to do so. In any case the work is a wonderful compendium: the index contains more than 7,000 names, and as for the figures, there are almost too many for the size of the book.

It is not only professed students of geology and palæontology proper that should be grateful for this work; but the zoologists also,

now that they have so ready a means of reference put within their reach, may occasionally be glad to pay some attention to the work of their colleagues on extinct animals, which they have hitherto been rather apt to treat as "out of sight and out of mind," to the serious detriment of their own work. There are, however, signs that this state of things is passing away, and one of them is the constant presentation of palæontological papers to societies or periodicals that deal with zoology in preference to those that deal with geology. Another sign is the gradual incorporation of palæontological matter in text-books of zoology, and the preparation of such works as Dr. Bashford Dean's book on fishes, reviewed in our last number. We hope the day will come, we believe it is not far distant, when the text-book of palæontology will be a thing of the past, and when the zoological text-book will treat all animals alike, whether they be living or extinct. The errors into which zoologists have been led through the habit that the circumstances of their training and the facts of nature rendered inevitable, the habit, namely, of treating as typical representatives of the various sub-divisions, forms that the palæontologist sees to be aberrant, degenerate, or extremely modified—these errors, we say, have left their trace in almost every branch of zoology, and it will be long before a truer conception of the relationships of the animal kingdom can gain a hold on the minds of men. Nobody is to be blamed for this: it was the obvious path for science to take, but she has been led by it along an obscure and a devious route.

The wedding-day of palæontology and geology has, however, not yet arrived. The engagement is likely to prove a long one, and in the meanwhile we may profitably employ our time in studying the works of Professor von Zittel. It is, therefore, good news to hear that an English translation of the present book is being prepared by Mr. C. R. Eastman, of Harvard, and that the translation is being revised and added to by numerous specialists, both English and American. It is, we believe, to be issued in two volumes, of which the first may be looked for very shortly.

A Dutch Palæontology.

De Gewervelde Dieren van het Verleden. By Dr. T. C. Winkler. Pp. vii. 291, illustrated. Haarlem, 1893.

We have received from Dr. T. C. Winkler, the well-known curator of the Teyler Museum, Haarlem, a copy of the above popular work, "The Vertebrate Animals of the Past." Though dated 1893, we have not previously had the opportunity of perusing this interesting volume, which is admirably adapted to give the Dutch public a general idea of the results of vertebrate palæontology. Holland is fortunate in possessing perhaps the richest of the smaller palæontological museums of Europe, the institution over which Dr. Winkler has presided for so many years. Here is preserved the original "Homo diluvii testis," and it is accompanied by numerous other unique examples of fossil vertebrata. The present volume naturally makes special reference to these specimens and to the writings of Dr. Winkler himself; it is precisely the handbook to be read by anyone interested in fossils after seeing the Teyler collection. There is a copious index and numerous woodcuts; but the latter are very inferior, and might well be replaced by "process" copies of similar blocks published elsewhere. Were we reviewing the book we should differ from the author on a few points; but on the whole it was remarkably up to date at the time of its issue three years ago.

MR. BRIGGS BECOMES NATURALIST.

BY TANGLED PATHS. By H. Mead Briggs. 8vo. Pp. 203. London: F. Warne and Co., 1896. Price 3s. 6d.

WE liked Mr. Briggs better in the old days, as the plain *paterfamilias*, the friend of Mr. Punch. But now, Mr. Briggs has become degenerate: he has joined the Natural History Club of the South-Eastern College, Ramsgate; he has swilled draughts of Albert Chevalier and the Poet Laureate; he has poured an uncooked mixture of his scraps into the columns of the *Kentish Gazette*; and, worst of all, he has, by a charmingly got-up reprint of his essays (for which all credit is due to the publishers) enticed us into reading them for purposes of review.

His rambles have indeed led poor Mr. Briggs into tangled paths. So long as he sticks to matter of fact we cannot complain: "Nature has many moods," "Death is no respecter of persons," "Colds are at all times undesirable." "Something is wrong somewhere"—true, O Briggs! But we cannot fully subscribe to such statements as these: "Everywhere in winter one finds treachery," "the reaper putteth in his sickle and the blade [*sic*] is severed." The kingfisher's eggs "blush with lovely pink lent by the conscious [!] yolk within." At Folkestone, "along the heights to this very day

'In the whirlwind o'er the spray
They behold the halcyon play.'"

The old naturalists did, it is true, suppose that kingfishers "sat brooding on the charmed wave," but it was reserved for a Briggs to see them above the breakers of the Channel. But then Mr. Briggs does wonderful things: he can hear a bee's heart beating, as we infer from a statement on p. 14; he knows that hedgehogs dream all the while they hibernate, they dream "of golden summer days, when the nightingales sang love melodies"; he knows that "the poet's Lorelei sat on the beetling heights above the sea, luring the sailor and his ship to destruction on the cruel rocks below,

'while softly flowed the Rhine.'"

Ah, yes! the poets! Mr. Briggs is fond of poets. We wonder if they are fond of Mr. Briggs, for he drags them into all impossible connections, not without abundant mis-quotation, even laying impious hands on "Hymns Ancient and Modern," as thus: "The strange wild cry of the wandering owl echoes back amid the deepening gloom,

'O'er moor, o'er [*sic*] fen, o'er crag, or [*sic*] torrent till
the night is gone.'"

or again: "birds of gloom,

'Like the shadows of the evening,
Softly steal across the sky.'"

Mr. Briggs is indeed a veritable Malaprop: he talks about "the shores of Babylon"; he calls an owl "the goose with the golden egg"; he speaks of the chaffinch's "feathered vocalism," as though it sang with its head under the bedclothes; he thinks that "where ignorance is bliss, etc.," is an old proverb; in parody of Shakespeare he tells us that "the blackbird tuned his orange bill"; he adds to the English language the words "discomforture" and "squallid"; and he thinks that "Revolted daughters" have grown so "mannish" that they will "beget" children who "will probably never know their parents."

The readers of the *Kentish Gazette* take their Briggs in instalments; but who shall swallow this warmed-up farrago of a book? Never mind! they that have the courage to attempt it may get many another jest from its pages, not all examples of conscious

humour perhaps. Even this solitary witticism may be unintentional: "Socialists are *radically* wrong." The italics are ours. Well, well! "Everybody ought not to be the same—equal. Nature never intended it." True again! but then those who are not equal to it should not publish. Let Mr. Briggs return to the bosom of his family and the illustrated pages of our humorous contemporary.

New Serials.

A new quarterly journal, *Centrallblatt für Anthropologie, Ethnologie, und Urgeschichte*, edited by Dr. G. Buschau, with the co-operation of eminent anthropologists, is issued by J. U. Kern, of Breslau.

Volume i. of the *Reports* of the University Geological Survey of Kansas, to whose energies we have more than once alluded, is announced. It contains about 100 pages of text, with figures and plates, and deals chiefly with stratigraphy of Carboniferous rocks in Kansas.

The *Psychological Review* announces the establishment of a new Russian monthly review of psychiatry, neurology, and experimental psychology, edited by Dr. Bekhteret.

For the study of the districts of Altaï and Nertchinsk there has been formed a Geological Section of the Cabinet of the Czar, under the direction of Professor A. Inostrantzev. This Section publishes its results under the title *Travaux de la Section Géologique*, etc. (Trud'i gheologhīcheskoï Kabīneta egho īmperatorskagho Velīchestva). Vol. i., Livr. 1, containing 144 pages, and Livr. 2, containing 94 pages, were published, in 8vo, at St. Petersburg in 1895. They contain papers, written in Russian, by P. N. Venyukov, A. A. Baïkov, V. I. Pletner, and the editor, as well as a geological and geographical bibliography of the Altaï.

From S. Paulo, in Brazil, comes vol. i. of *Revista do Museu Paulista*, publicada por H. von Ihering, 1895. It is in 8vo, and consists of 254 pp. and 3 plates, containing a description of the museum and other papers by von Ihering, P. Taubert, and A. Lutz.

The *Halifax Naturalist*, a bi-monthly magazine of natural history and archæology of the parish of Halifax, Yorkshire, deserves a welcome, if only for the brightness of its general appearance. The first number contains: a geological map of the parish on the scale of 1 inch to the mile, compiled from the Ordnance Survey; an account of some physiographical features of the parish of Halifax, by W. Simpson; an introduction to the flora of the parish of Halifax, which will form, when finished, an independent work; a record of the meetings of the Halifax Scientific Society; and various smaller notes. The editor is Mr. W. B. Crump, and the price is 6d. This magazine will deal with the natural history of the ancient parish of Halifax, and will no doubt tend to increase the interest in natural history that is already felt by a number of the inhabitants. While we appreciate the merits of this first number, we must again protest against the increase of local publications. There already exists the *Naturalist*, a monthly journal of natural history for the North of England, which, as it so constantly contains articles connected with Yorkshire, should have sufficed the needs of Halifax.

The first number of the *Idaho Mining News* (Boise, Idaho, price $1 per annum) appeared in March. According to the *American Geologist*, it contains papers on the Boise gold belt and the coal-fields of the Payette River.

A monthly *Index* to the medical press of the United States and Canada is announced by Frank Weir & Co., New York.

No. 1 of the *Journal of Experimental Medicine* contains 210 pages, eight plates, and other illustrations. The journal will be devoted to original investigations in Physiology, Pathology, Bacteriology, Pharmacology, Physiological Chemistry, Hygiene, and Medicine. It is published for Johns Hopkins University by D. Appleton & Co., New York. Subscriptions, $5 per volume of over 600 pages, may be sent to Mr. N. Murray, Johns Hopkins University.

OTHER SERIALS.

THE January number of the *Psychological Review* contains a valuable paper on "Psychology and Physiology," by Professor G. S. Fullerton, of Pennsylvania.

The January number of the *Monist* publishes a translation by T. J. McCormack of Professor Weismann's address on "Germinal Selection" delivered before the International Congress of Zoologists last September; also a translation of Professor Ernst Mach's recent inaugural address on the part played by accident in invention and discovery.

A very timely paper by Dr. James Rodway on "the old Boundary of Essequebo" is to be found in the December number of *Timehri*, which also contains "Stray Notes from Pirara" and "Some Guiana Parrots" by C. A. Lloyd, and an investigation into the materials of the urali poison by J. J. Quelch.

Vol. viii., pt. 2, of the *Journal of the College of Science* at Tokyo, which has been sent to us, contains a paper by Asajiro Oka, which describes *Orobdella*, a new genus of land-leech found in Japan. The same author has a note on the so-called excretory organ of fresh-water Polyzoa, in which he comes to the conclusion that the "Ectoproctous Polyzoa have no nephridia. What have been regarded as such in Phylactolæmata is nothing but a portion of the mesodermal epithelium of the body-cavity, made conspicuous by the presence of an epistome-lophophoral partition, wanting in Gymnolæmata." Sakugoro Hirase contributes "Études sur la Fécondation et l'Embryogénie du Ginkgo Biloba." Dr. I. Ijima and S. Ikeda describe a new species of the peculiarly shaped octopod, *Opisthoteuthis*, under the name *O. depressa*. It is remarkable that the only other species of this genus that is known was found in the West Indies; in our January number, p. 10, we recorded a somewhat similar distribution of the Chimæroid *Hariotta*.

The March number of the *American Journal of Science* contains an article on recent and fossil tapirs by J. B. Hatcher, which, besides adding to our knowledge of the Oligocene *Protapirus*, reviews the general question of the phylogeny of the Tapiridæ and Helaletidæ.

In the April number, C. E. Beecher gives a restoration of the dorsal and ventral surfaces of *Triarthrus*; Dr. A. E. Ortmann criticises the arguments advanced by Neumayr for the existence of climatic zones in Jurassic times, and comes to the conclusion that the differences in the faunas of the Jurassic deposits were not caused by climatic differences, but partly by topographical conditions, and partly by differences of depth and of sea-bottom in the seas.

Science for February 21 contains an interesting address by Dr. Theo. Gill on "Huxley and His Work." In this the passage of most original interest is that where the learned author credits to Huxley's sagacity the early appreciation of the affinity of the *Neoceratodus* of Australia to the Mesozoic Ceratodontidæ, with all the far-reaching consequences that appreciation involved.

L'Anthropologie for January and February contains a paper of considerable interest to botanists by Edouard Piette, "Les Plantes cultivées de la Période de Transition au Mas-d'Azil," which place is on the River Arise.

A short sketch of the history, geography, and industries of Venezuela is to be found in the *Scottish Geographical Magazine* for April, which also contains an interesting paper by James Troup, H.B.M. Consul at Yokohama, on the industrial and commercial development of Japan. Very timely also is a long account of Cuba extracted from the articles by Dr. Emil Deckert that have recently been published in the *Geographische Zeitschrift*.

In connection with the acceptance by Dr. J. M. Coulter of the head professorship of Botany at Chicago University, the financial management of the *Botanical Gazette*, of which he is senior editor, passes into the hands of that body. The immediate result is an increase in number of pages, and after July the *Gazette* will be published at the University of Chicago, whither all correspondence should now be addressed. The editorial staff is unchanged. The *Gazette* will be "even more freely open to the botanists of the world than it has been in the past." Its prosperity, as well as its excellence, is now assured.

The April number of *Knowledge* contains a thoroughly practical article by J. W. Gifford, entitled "Electrography or the New Photography," which has some excellent reproductions of electrographs, actinograms, or skiagrams. Other skiagrams are reproduced in the *American Naturalist* for March, in the *Photogram* for April, in *Nature* for January 23, February 6, and March 5, in the *Revue Scientifique* February 1, and in the *Revue Générale des Sciences* January 30; but perhaps some of the best are those that were given in *The Open Court* for February 6 and March 12.

Messrs. Williams & Norgate have kindly sent us their *Foreign Book Circular, Scientific Series*, no. 63. It is stated to be a "list of new scientific publications," and contains 572 titles. Since, however, fully half the publications seem to be older than 1895, and since no reference is made to the fact that many of them are merely extracts from serials, we cannot think that the list will be of much use to either librarians or working naturalists.

From the Free Public Reference Library of Nottingham we receive the third supplement to the list of scientific books and periodicals, compiled, under the direction of J. Potter Briscoe, by S. J. Kirk, senior assistant, price 1d. From this and previous lists it appears that the library contains a very useful selection of scientific works, especially rich in those relating to the neighbourhood. No foreign scientific periodicals are placed on the tables, and we notice in this list scarcely any books published outside Great Britain.

Literature Received.

Primary Factors of Organic Evolution. E. D. Cope: Open Court, Chicago. British Sea-Birds, C. Dixon: Bliss, Sands & Foster. Ethnology, A. H. Keane: Clay. Natural History of Selborne, G. White: Ginn (Boston). Royal Natural History, nos. xxix., xxx.

Iowa Geological Survey, vol. v.: Administrative Reports. Catalogue de Botanique: Balllière. The Jack Rabbits, T. S. Palmer: *Bull. U.S. Dept. Agric.* Australian Termitidæ, pt. i., W. W. Froggatt: *Proc. Lin. Soc. N.S.W.* Annual Report Geol. Survey Great Britain, 1895. Physiology of the Root Tubers of Isopyrum biternatum, D. T. MacDougall: *Minn. Bot. Studies.*

Actes Soc. Sci. Chili, t. v. Science Progress, April. Micros. Studies in Botany, March. Nature, March 19, April 9, 16. Knowledge, April. Review of Reviews, April. Revue Scientifique, March 21, 28, April 4, 11. Irish Naturalist, April. Revue générale des Sciences, Feb. 29, March 15. Feuille des jeunes Naturalistes, April. Naturen, March. American Journal of Science, April. Journal of Malacology, vol. v., no. 1. Naturæ Novitates, March. Victorian Naturalist, Jan. and Feb. Science, March 13, 20, 27, April 3. Scottish Geographical Magazine, April. Science Gossip, Jan. The Naturalist, April. Westminster Review, April. American Geologist, April. Botanical Gazette, March. Biology Notes, March. Scientific African, March. Psychological Review, Feb. The Photogram, March and April.